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# Trajectoires professionnelles et santé en Europe

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# Essays on careers and health in Europe

## T H È S E

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# General Introduction

## General motivation

Health and safety at work is one of the most important aspects of the EU policy in the field of employment and social affairs. Over the last decades, the adoption and implementation of a substantial corpus of Community legislation[1] have improved working conditions in the Member States and have already borne fruit : workplace accidents have been markedly fewer in number. Member States have acknowledged under the Lisbon Strategy that health and safety policy makes an important contribution to economic growth and employment.[2] The 2007 council resolution on a new community strategy on health and safety at work (2007-2012) stated that "occupational safety not only safeguards workers' life and health [...] but also plays a vital role in increasing the competitiveness and productivity of enterprises and in contributing to the sustainability of social protection systems by reducing the social and economic costs of occupational accidents, incidents and diseases."

Work-related injury and illness challenge health systems' ability to preserve and restore the capacity of workers to maintain economically active. This is particularly relevant in the light of the ongoing demographic, as EU populations are rapidly ageing. Increasing the economic activity of older workers is considered essential to relieving the economic pressures generated by demographic trends as well as trends towards early retirement. As a result, half of OECD countries are raising retirement ages or will do so in the coming decades, and most governements try to reduce retirement incentives. However, these measures will not necessarily be effective, especially if work is too health-demanding at old age[3] or if ill health is a motive of labour market

[1] Based on Article 137 of the EC Treaty of Nice.

[2] Workplace accidents and work-related illnesses are costly in not only human but also economic terms. Every year there are more than 4 million accidents at work in the EU. In macroeconomic terms the cost of accidents at work and of occupational diseases in EU-15 ranges from 2.6% to 3.8% of gross national product (EUCommission, 2007).

[3] On this topic, the 2007 council resolution on a new community strategy on health and safety at work (2007-2012) stated that "workplaces must be designed in such a way that the employability of workers is ensured

withdrawal at old age. New risks at the workplace – such as the risks arising from new forms of work organisation or from the intensification of work pressure – make this issue even more critical.
More generally, good health at work helps improve public health. Recognizing that occupational health is closely linked to public health and health system development, the World Health Organization developed a Global Plan of Action on Workers' Health (2008-2017). The plan seeks to address all determinants of workers' health, including risks of disease and injury in the occupational environment, social and individual factors, and access to health services. It also aims at reducing inequalities in workers' health – as some groups, such as temporary workers, immigrants, disabled and young and old workers are at greater risk of suffering from poor health and safety conditions at work.

Beyond workplace health and safety, a new policy challenge lies in the health impact of more insecure careers on the labour-market. The growth of precarious work since the 1970s – i.e. employment that is uncertain, unpredictable, and risky from the point of view of the worker – has emerged as a core contemporary concern. Uncertain and unpredictable work contrasts with the relative security that characterised the three decades following World War II (Kalleberg, 2009). Although the EU legislation is much less developed on this topic, the health consequences of precarious careers are likely to be large, too. Various career shocks over the lifecourse may be harmful to health, and may challenge health systems' ability to preserve and restore the capacity of workers to remain in the labour force. The main objective of this thesis is to analyse the health consequences of career shocks.

### Is work (that) bad for health?

Whether and to what extent work may affect health is an open question, however. Of course, various aspects of work may be a hazard and pose a risk to happiness and health. Cases of burnout, or even suicides at work regularly make the headlines of the press. Several tragic episodes, such as the asbestos scandal, have proved that harsh working conditions may damage health, and even kill. But we also know that job loss and unemployment may have severe consequences on health, too. This apparent contradiction is far from new. The XVIII$^{th}$ century initiated this still ongoing debate, opposing two views of work : work as a contribution to

throughout their working lives. At the same time, workplaces should be tailored to the individual needs of older and disabled workers."

human progress, the foundation of social ties and a source of fulfillment and happiness; work as an alienating job, that condemns individuals to waste their life trying to earn one's living.

Employment is generally the most important means of obtaining adequate economic resources. It is also important to meet important psychosocial needs in societies where employment is the norm; finally, it is central to individual identity, social roles and social status (Waddell and Burton, 2006; Layard, 2004; Dodu, 2005; Nordenmark and Strandh, 1999). In a French survey on happiness (1996-1999), one person out of four answered "work" – or closely-related concepts such as "profession", "job" etc. – to the following question : "What is the most important factor to your happiness?".[4] This proportion amounted to 65% among unemployed individuals or individuals predicted to be at a high risk of unemployment, e.g. blue-collar workers under 35, and temporary workers (Baudelot et al., 2003). Correspondingly, the subjective value of work has steadily increased in France during the last decades. French unemployment rates have not fallen below the level of 8% since 1983, and polls keep reminding us that French people consider employment as the first priority on the political agenda.

Richard Layard says that "when a person becomes unemployed his welfare falls for two reasons – first the loss of income, and second the loss of self-respect and sense of significance (the psychic loss). The pain caused by the loss of self-respect is (we find) at least as great as the pain which a person would feel if he lost half his income. So unemployment hits with a double whammy – the loss of the income hurts, but so does the loss of self-respect". Job loss can be a highly traumatizing event indeed. Sociologist Paul Lazarsfeld and his team grasped evidence of this as soon as in the 1930s, in the small Austrian town of Marienthal (Lazarsfeld et al., 1933). The mine, which was then the principal economic resource in the town, was forced to close in the wake of the 1929 economic crisis. Social life, which was particularly rich before the crisis, drastically declined after the plant closure and symptoms of "apathy" emerged afterwards among long-term unemployed individuals.

Beyond happiness and welfare, there is evidence that losing one's job is also detrimental to health. Sullivan and Von Wachter (2009) consider high-tenure male workers displaced during the early and mid-1980s in the course of mass layoffs in Pennsylvania. They show that they experienced a 50 to 100% increase in the mortality hazard during the immediate years following job loss. The effect decreased as time passes but converged to a 10-15% increase in the long

[4] The question in French was labelled as "Qu'est-ce qui est pour vous le plus important pour être heureux ?"

run.[5]

Overall, job loss and unemployment are likely to be detrimental to health. However, we should not be too hasty in concluding that work is beneficial to health.

That work is likely to deteriorate health is quite a widespread idea. When asked how work influences their health, 25% of Europeans declared work to be pathogenic. Conversely, only 7% reported it to be positive to their health (EWCS 2010, reported in Barnay (2014)). Of course, these subjective figures reflect various norms regarding effort, health and work, but they indicate that the objective health-damaging impact of work is likely to be non-negligible.

Public health and epidemiology have long suggested that various dimensions of work were likely to be harmful to health. As far back as in 1840, Dr. Villermé depicted the harsh working and living conditions of workers in French textile centers in Lille, Rouen and Lyon. His "Tableau de l'état physique et moral des ouvriers employés dans les manufactures de coton, de laine et de soie" reported that many textile workers were living in insanitary conditions. Of all the workers Villermé observed, the handloom weavers ("tisserands à bras") fared the worst. Villermé vividly described their bad health, attributing it to harmful working conditions, long working hours, and inadequate nutrition (La Berge, 2002). His "Tableau" – together with his various reports on work accidents – were the foundation of laws on health at work, medical surveillance of occupational risks.[6] Today, various surveys of health at work[7] – continue to provide data to explore the work-health connection.

Smith (2004) notes that medical scientists are often convinced that the dominant situation is that working conditions, psychosocial aspects of work and career outcomes produce large health disparities. Their main debate is about why they lead to poor health. That a reverse causation may be at play – e.g. health affects the capacity to work or even working conditions – is often ignored in empirical studies. These endogeneity issues – which mostly come down to selection issues – are at the core of the empirical analysis of work and health. We come back to this question in the methodology section.

[5] Bassanini and Caroli (2014) provide a recent and substantial review of the literature on the health impact of job loss. They find that "the results are mixed, ranging from strong health damaging effects to insignificant ones. However, no article ever finds a positive health effect of becoming unemployed."

[6] In particular, Villermé's work was instrumental in passing a French 1843 law preventing children under 8 to work in factories with more than 200 employees.

[7] In the French context, see for instance the SUMER survey on the medical surveillance of exposure to occupational risks, the ESTEV survey on health, work and ageing or the GAZEL survey. For Europe, see for instance the health module of the European Working Conditions Survey (EWCS).

Overall, the health impact of work has received considerable attention by the past. Does this question even matter today? There are some reasons to wonder. Work is indeed less dangerous than it used to be. In France, work accidents have declined by a third between 1955 and 1975 – twenty years characterised by strong economic growth and a modernisation of the industrial equipment (DGT, 2011). The study of whether and to what extent work may affect health, however, needs our attention more than ever. The speed of work has quickened and the pressures of working to tight deadlines have also risen. At the same time, popular concern about precarious work and insecure careers has increased. Both the growing intensification of work pressure and career insecurity may be detrimental to health.

## The health consequences of a changing world of work

### Growing intensification of work pressure, and health

There is evidence that the speed of work and the pressures of working to tight deadlines – as reported by workers in surveys – have risen since the 1980s. This growing intensification of the work pressure (which may originate from new forms of organisation inside firms – such as the decentralisation of authority, delayering of managerial has functions, and increased multitasking – or from the introduction of information and communication technologies) has been hypothesised to affect worker's health. The relationship between stressful working conditions, psychosocial aspects of work and the possible development of pathologies has mainly been analyzed with two theoretical frameworks : the Effort-Reward Imbalance Model initially developed and tested by Siegrist (1996) and the Job Demand Control Model of Karasek (1979) – see for instance Ferrie et al. (1998); Askenazy et al. (2006) for empirical evidence of the health-damaging impact of work pressure and organisational change.

### Career insecurity and health

In parallel, popular concern about job security has increased in a large number of industrialized countries over the past thirty years. Newspapers and other popular sources described the impact of major firm downsizings and changes in workers' perceived job security (Valletta, 1999). Careers indeed seem to be more uncertain and unpredictable than they used to be. Unemployment spells and temporary work are more common than ever. Careers are particularly insecure among the youth. In 2013, the unemployment rate for the 15-24 in the OECD was equal to 16.2% – as compared to 7.3% for prime-age workers. It was as high as 20.9% in the UK, 23.9% in France, 40% in Italy and above 50% in Spain and Greece. Only 16.7% of prime-age individuals were in their job for less than 12 months in 2013, while this proportion amounted to 50.4% among the youth. At the other end of the age spectrum, elderly workers have particularly few job opportunities. In the OECD, employment rates of those aged 55-64 – 56.4% in 2013 – were particularly low. The incidence of long-term unemployment (12 months and over) among people aged 55 and over was as high as 43.8%. Most elderly workers prefer to take early retirement routes, sometimes after unemployment or disability periods. In-between these two critical periods, permanent-contract holders represent the main bulk of prime-age workers in the OECD (83% in 2013). However, the incidence of long-term unemployment increases with age (38.4% for the 25-54), so that finding another job or even finding a job with a similar salary if fired or quitting one's job is likely to be difficult.

Overall, this growing career insecurity is likely to deteriorate health. The recent success of "Deux jours, une nuit", a French film by Jean-Pierre and Luc Dardenne is evidence of this concern. This award-winning film stages Sandra, a young woman who works in a small factory. Sandra suffers from a nervous breakdown and is forced to take time off from her job. In her absence, her workmates realise that are able to cover her shifts by working slightly longer hours. The management offers a significant pay bonus to each member of staff if they agree to make Sandra redundant. In this film, the link between health and career insecurity is particularly well depicted : bad health leads to more insecure careers, and even job loss; conversely, career insecurity is detrimental to health – or at least increases depressive symptoms, as Sandra attempts suicide once she learns that she may lose her job.

## Objective of the thesis

The main objective of this thesis is to analyse the health consequences of career shocks.

We first focus on two critical periods over one's career : the entry on the labour market and, at the other end of the age spectrum, retirement. The first chapter considers low-educated individuals in England and Wales who left full-time education in their last year of compulsory schooling immediately after the 1973 oil crisis. Recent labour economic research shows that poor macro-economic conditions at labour-market entry lead to persistent and negative career effects. This chapter investigates whether it also entails negative health consequences. This question is a topical issue in the current context. As young cohorts who left full-time education in the Great Recession faced historically high unemployment rates and experienced difficulties accessing employment, it will most likely generate health disparities in the future.

The second chapter considers another critical period in one's career, i.e. retirement. Retirement is the most common transition out of employment. Most older workers withdraw from the labour-force long before reaching the official retirement age, either because employment opportunities are too scarce, or because health problems make it difficult to exert or retain their jobs. We focus on individuals who take early routes to retirement and build on the large literature on the effects of retirement on health. The results in this literature are very ambiguous, and whether or not retirement has a detrimental effect on health is still an open debate. This is mainly due to the fact that analysing the long-term health consequences of retirement – which are not easily disentangled from the effect of age – remains a hard task. A promising way to solve this "retirement puzzle" is to look, as we do, at behavioural outcomes following retirement. These behavioural outcomes can be rapidly modified in the short-run and precede the longer-run health outcomes (such as chronic diseases, mortality etc.). We thus analyse how weight change is modified in the short-run. There is a good reason to look specifically at weight change and obesity, as they are indeed strong predictors of health at old age.

Our last chapter also deals with career shocks and health. The focus, however, is slightly different : we do not specifically focus on a critical period in one's career. Neither do we consider an *actual* or *realized* career shock. Rather, we investigate the health impact of the anticipation of a career shock, and more specifically, the anticipation of job loss. Psychologists have long shown that the anticipation of a stressful event represents an equally important or even greater source of anxiety than the event itself (Lazarus and Folkman, 1984). Although job loss is a highly traumatizing event, it is fortunately not very frequent. In contrast, the fear of involuntary job loss, i.e. perceived job insecurity, is likely to be much more widespread, and one may wonder whether its health impact is as negative as that of actual job loss.

## Methodology

Our methodology relies on three key elements. We first argue that health is probably better understood in a lifecourse perspective, and show that this thesis makes an attempt to build in that direction. A second feature of our methodology is that we pay special attention to the identification of causal relationships between career shocks and health. Finally, we use rich data from several surveys : this allows us to widen the geographical scope of our analysis and to consider a wide range of health indicators.

### A lifecourse approach

The lifecourse approach – which was first developed in epidemiology – focuses on the long-term effects on health of physical and social exposures during gestations, childhood, adolescence and young adulthood. Growing evidence suggests that there are critical periods of growth and development, not just in utero and early infancy but also during childhood and adolescence, when environmental exposures do more damage to health and long-term health potential than they would at other times (WHO, 2000). Recent and often insightful studies in health economics have indeed shown that socioeconomic circumstances during infancy and early-childhood years have a bearing on health outcomes and mortality later in life (Almond, 2006; Kesternich et al., 2014; Lindeboom et al., 2010; Van den Berg et al., 2006). Overall, circumstances early in life play a crucial role in determining the co-evolution of socioeconomic status and health throughout adulthood (Cutler et al., 2008). To assess the impact of early-life circumstances on later health outcomes is an empirical challenge. To investigate this question, life sciences have developed experimental set-ups – see for instance Herborn et al. (2014) and the extensive literature review by Gluckman et al. (2008). But economists have mainly used natural experiments. They exploit sources of independent variation in early-life circumstances that affects later health – but only via childhood conditions. This independent variation is typically provided by epidemics, famines, war episodes, the state of the business cycle (e.g. GDP variation) at birth etc.

The lifecourse approach is likely to improve our understanding of how health is determined at older ages. It also sheds light on how health disparities across socioeconomic status evolve over time (Cutler et al., 2008). Evidence of this new interest in the lifecourse can be found in Galama and Van Kippersluis (2010) : in an extension of the Grossman model of the demand for health (Grossman, 1972),the authors develop an innovative conceptual framework in which multiple mechanisms and their cumulative long-term effects can be studied in a structural model of socioeconomic status and health over the lifecourse. From an empirical point of view, however, data permitting to take such a lifecourse perspective are scarce : most data do not cover a sufficient time span to examine full lifetimes of individuals.

This thesis is a modest attempt to build in this direction. Our first two chapters focus on two critical periods in the lifecourse. The first chapter investigates how a shock during early adulthood – poor economic conditions at labour-market entry – affects health in the long run.

We exploit the 1973 oil crisis as an exogeneous shock on macroeconomic conditions at school-leaving. We use data from a repeated cross-section of individuals over 1983-2001 – from 7 to 26 years after school-leaving – to study how their health is impacted in the long-run. The second chapter focuses on another critical period in the lifecourse, i.e. retirement. Retirement is a critical period to health as it implies major changes in individual lifestyles. Overall, retirement is likely to play a major role in shaping post-retirement health.

### Identifying causal relations

This thesis studies the impact of career shocks on health in an empirical setting. More specifically, it exploits observational data from pan-European or British surveys. To assess the causal impact of work on health from observational data is not always an easy task. This is mainly due to endogeneity problems that plague the analysis. These endogeneity problems mostly come down to selection issues which can be either "static" or "dynamic" (Bassanini and Caroli, 2014). Static selection is known as "the healthy worker effect" : healthy workers are more likely to be in employment than unhealthy ones; they are also more able to work in jobs with adverse working conditions – see McMichael (1976); McMichael et al. (1974) for empirical evidence of this. Dynamic selection happens when changes in workers' health generate changes in their employment status or in the number of hours they work. Empirical evidence of such a dynamic selection can be found for instance in Smith (2004) and García-Gómez et al. (2013). These two articles provide convincing evidence that health changes – e.g. the onset of a chronic disease or an acute hospitalization – have non-trivial and long-lasting impacts on labour-market outcomes. García-Gómez et al. (2013) identify the causal effects of sudden illness, represented by acute hospitalizations, on employment and income up to six years after the health shock. The authors use a unique set of linked Dutch hospital and tax register data. Their identifying assumption is that acute hospitalizations are likely to be exogeneous to socio-economic status – including labour-market outcomes –, by virtue of being unexpected.[8] In addition, the authors take account of observable differences between employed individuals with and without an acute admission by using propensity-score matching and combine this with difference-in-differences (DiD) regressions to correct for any selection on time-invariant unobservables. They show that an acute hospital admission lowers the employment probability by seven percentage points and results in a 5 percent loss of personal income two years after the shock.

[8] This assumption is likely to hold as only individuals aged between 18 and 64 who had not been admitted in hospital in the previous year are included in the sample.

If not properly dealt with, these endogeneity problems – either static or dynamic selection – plague the empirical analysis and lead to biased estimates. This is a problem that we face in all chapters. Let us first consider the case of retirement. Retirement is often a choice, and there is indeed widespread evidence in the literature that workers with poor health status tend to retire earlier (Currie and Madrian, 1999). A simple correlation between retirement and health would lead us to overestimate the negative health impact of retirement. Now, turning to job insecurity, it may be the case that healthy individuals are more likely to be employed in insecure jobs, because they know that they will be able to get a desirable job if fired. If it is the case, a simple correlation between job insecurity and health would lead us to underestimate the negative health impact of job insecurity. Endogeneity problems also arise when we assess the impact of poor economic conditions on long-term health, although in a slightly different manner. In essence, time of school-leaving may be endogeneous to the contemporaneous economic conditions, so that pupils leaving school at compulsory age may be selected. On the one hand, school-leavers who avoid leaving school in a bad economy may have unobserved characteristics (e.g. parental socio-economic characteristics) that allow them to postpone their entry on the labour market. On the other hand, it is likely that only the most capable and hardworking types are able to leave school during a bad economy since their abilities allow them to secure desirable jobs regardless of the economic conditions. If, by any chance, these characteristics are correlated with subsequent health, our estimates will be biased.

In the past three decades, a counterfactual model of causality has been developed, and a unified framework for the study of causal questions is now available (Morgan and Winship, 2014). A wide range of techniques are now widely used in applied economics to answer simple cause-and-effect questions : matching methods, instrumental-variable models, differences-in-differences regressions etc. We use these econometric tools – instrumental-variable methods, typically – to deal with these causal inference problems. In each chapter, we try to come up with a convincing identification strategy to provide causal estimates. We use natural experiments – the 1973 oil crisis for instance – as well as the the variation in institutional features to exert exogeneous shocks on careers.

### Surveys and health indicators

We exploit data from three different surveys : the British General Household Survey (GHS), the Survey of Health, Ageing and Retirement in Europe (SHARE) and the 2010 European Working Conditions Survey (EWCS). Each survey focuses on the specific population relevant to our analysis : individuals in Great-Britain over 1972-2011 (Chapter I), persons aged 50 and over across European countries over 2005-2010 (Chapter II); and persons in employment in 2010 in Europe (Chapter III).

These various surveys allow us to consider a wide range of health indicators. Of course, measuring health using survey data is always a challenge : various sources of bias may affect the assessment of health, such as reporting and justification biases (Barnay, 2014). This section provides a brief overview of these surveys and discusses the various health indicators used in the thesis.

**The General Household Survey.** Our first chapter exploits British data from the General Household Survey (GHS). The GHS is an annual survey of over 13,000 households and a nationally representative survey of private households in Great-Britain. It ran from 1972 to 2011 as a repeated cross-sectional survey. The GHS is a new cross-section in each year, so that although we cannot track any individual over time, we can track birth cohorts. This survey is particularly adequate for to our analysis since a number of GHS respondents left full-time education immediately after the 1973 oil crisis. We use the 1983-2001 survey waves and take a life-course perspective, from 7 to 26 years after school-leaving. The GHS questionnaire included health measures as far back as the 1970s, although some measures were inconsistent over the years. It contains information on health, health care and health behaviours. We use a measure of self-reported health status. Respondents are asked to rate their health on a 3-point scale : good, fair or bad. We dichotomise the responses into good and bad health (fair or bad). There is evidence in the literature that self-rated health is a good indicator of individual overall health (Ferrie et al., 1995). It has been found to be a good predictor of mortality even after controlling for more objective measures of health (Idler and Kasl, 1991; Bath, 2003). However, the probability of reporting good or bad health may suffer from individual reporting heterogeneity (Tubeuf et al., 2008; Etilé and Milcent, 2006). This is why we also include more objective measures of health, such as the presence of a longstanding illness. These health indicators measure rather severe conditions, and are particularly well-suited when one is interested in the long-run. Finally, the GHS includes measures of health care (GP and hospital consultation) as well as health behaviours (smoking and drinking behaviours). To the extent that reporting bias in self-reported health measures remains the same across individuals regardless of economic conditions at labour-market entry, it should not bias our analysis.

**The Survey of Health, Ageing and Retirement in Europe.** The Survey of Health, Ageing and Retirement in Europe (SHARE) is a multidisciplinary and cross-national panel database containing individual information on health, socio-economic status and social and family networks. This cross-country dimension is particularly interesting as it gives us the opportunity to exploit the European variation in retirement systems to design a neat identification strategy. Approximately 85,000 individuals over 50 years old and their spouses/partners (independent of their age) from 19 European countries (including Israel) have been interviewed so far. By now, four waves have been conducted and further waves are being planned to take place on a biennial basis.[9] These panel data are particularly relevant when one is interested in transitions across waves – retirement, typically – and their short-term consequences. SHARE includes a wide range of health indicators, which are measured in a consistent way across waves. The Body Mass Index (BMI) is calculated in each wave as the self-declared weight in kilograms divided by the square of the self-declared height in meters (kg/m2). The BMI is a rather crude measure of body composition, as it does not distinguish fat from lean mass (Prentice and Jebb, 2001; Burkhauser and Cawley, 2008). However, it has been shown to be highly correlated with more precise measures of adiposity. When reported – as it is the case here, the BMI may additionally suffer from measurement error (Niedhammer et al., 2000; Burkhauser and Cawley, 2008). Following Brunello et al. (2013), we note that the rank correlation between country level self-reported and objective measures of weight is however very high in Europe (Sanz de Galdeano, 2007). We summon the existing literature to show that reporting bias in BMI is not likely not vary with retirement behaviour.

**The 2010 European Working Conditions Survey.** Since its launch in 1990, the EWCS measures and monitors trends and changes in working conditions in Europe. It has been conducted every five years on a random sample of workers (salaried employees and self-employed) in a growing number of European countries (from 12 in 1990 to 34 in 2010). The European Foundation for the Improvement of Living and Working Conditions commissioned the fifth wave of the EWCS to be carried out in winter-spring 2010. This single cross-section is particularly relevant to our analysis of job insecurity as it is a snapshot of persons in employment across Europe. Its European dimension allows us to exploit the cross-country variation in Employment Protection Legislation (EPL) to develop an original identification strategy. In 2010, the EWCS survey introduced a detailed health module. Previously, the EWCS had only a few questions

[9] A fifth wave has been made available to researchers by the beginning of April 2015.

related to health, such as “Do you think your health or safety is at risk because of your work?” or “Does your work affect your health, or not?”. The formulation of these questions is problematic, as it is likely to suffer from framing effects. This is why we do not use EWCS data prior to the 2010 wave. The 2010 EWCS module provides information on self-reported health status (not work-related) as well as on more objective measures of health capturing specific diseases or symptoms. In the EWCS database, respondents are asked whether they have suffered over the last 12 months from either backache, skin problems, muscular pain in shoulders, neck and/or upper limbs, muscular pain in lower limbs, headache or eyestrain, stomach ache, cardiovascular diseases, depression or anxiety, overall fatigue, or insomnia or general sleep difficulties. Most of these health symptoms are mild – except cardiovascular diseases – and likely to be affected in the short-term by job insecurity.

## Outline of the thesis

This thesis analyses the health impact of three key career shocks – anticipated or not. The first chapter focuses on the entry on the labour market in a bad economy. At the other end of the age spectrum, the second chapter deals with retirement, while the last chapter considers job insecurity in prime age.

### Chapter I

Our first chapter investigates whether leaving school in a bad economy deteriorates health in the long-run. It focuses on individuals in England and Wales who left full-time education in their last year of compulsory schooling immediately after the 1973 oil crisis. Our identification strategy relies on the comparison of very similar pupils – born the same year and with a similar quantity of education (in months) – whose school-leaving behaviour in different economic conditions was exogeneouly implied by compulsory schooling laws. This original identification strategy is different in spirit from the ones previously used in the literature. Rather than considering long periods of economic fluctuations and exploiting the variation in country (or state) school-leaving unemployment rates, we focus on two birth cohorts only – the 1958 and 1959 cohorts. As a consequence, our results cannot possibly be biased by country-specific (or state-specific) cohort effects. Unlike school-leavers who did postpone their entry on the labour market during the 1980s and 1990s recessions, we provide evidence that pupils’ decisions to leave school at compulsory age immediately after the 1973 oil crisis were not endogeneous to the contemporaneous economic conditions at labour market entry. We use a repeated cross section of individuals over 1983-2001 from the General Household Survey (GHS) and take a life-course perspective, from 7 to 26 years after school-leaving. Our results show that poor economic conditions at labour-market entry are particularly damaging to women's health. Women who left school in a bad economy are more likely to report poorer health and to consult the General Practitioner over the whole period under study (1983-2001). Additional evidence suggests that they are also more likely to suffer from longstanding illnesses. As for men, the health impact of poor economic conditions at labour-market entry is more mixed. Men who left school in a bad economy seem to be negatively affected in various dimensions (smoking status, and to some extent health status), although these effects are not robust across all specifications. Finally, we do not find any significant effects of poor economic conditions at labour-market entry on subsequent labour-market outcomes from 7 to 26 years after school-leaving.

### Chapter II

Our second chapter contributes to the literature on retirement and health. Its originality lies in the fact that it considers a behavioural outcome not much studied, i.e. weight. Weight change and obesity are strong predictors of health at old age and can be rapidly modified following retirement. We estimate the causal impact of retirement among the 50-69 year-old on Body Mass Index (BMI), the probability of being either overweight or obese and the probability of being obese. Based on the 2004, 2006 and 2010-11 waves of the SHARE survey, our identification strategy exploits the European variation in Early Retirement Ages (ERAs) and the stepwise increase in ERAs in Austria and Italy between 2004 and 2011 to produce an exogeneous shock in retirement behaviour. Our results show that retirement induced by discontinuous incentives in early retirement schemes causes a 13 percentage point increase in the probability of being obese among men within a two to four-year period. We find that the impact of retirement is highly non-linear and mostly affects the right-hand side of the BMI distribution. Additional results show that our results are driven by men having retired from strenuous jobs and who were already at risk of obesity. No effects are found among women.

### Chapter III

Our last chapter estimates the causal effect of job insecurity on health. To our knowledge, we are the first to provide such a causal estimate in the literature. We improve on the literature by using an instrumental-variable strategy which allows us to control for both time-invariant and time-varying omitted variables and/or reverse causality. We rely on an original instrumental variable approach based on the idea that workers perceive greater job security in countries where employment is strongly protected by the law, and relatively more so if employed in industries where employment protection legislation is more binding, i.e. in industries with a higher natural rate of dismissals. Using cross-country data from the 2010 European Working Conditions Survey, we are able to identify the causal impact of perceived job insecurity and show that job insecurity triggers mild health symptoms. Even in the short-run, the fear of unemployment gives rise to headaches, eyestrain as well as stomach ache.

#### Outline

This thesis is organised as follows. Chapter 1 explores the link between poor macro-economic conditions at labour-market entry and health in the long-run. Chapter 2 presents our analysis on the impact of retirement on weight. The last chapter provides evidence that job insecurity can be harmful to health. The final section concludes.

# Chapter 1

# The lasting health impact of leaving school in a bad economy.


## Abstract

This paper investigates whether leaving school in a bad economy deteriorates health in the long-run. It focuses on individuals in England and Wales who left full-time education in their last year of compulsory schooling immediately after the 1973 oil crisis. Unemployment rates sharply increased in the wake of the 1973 oil crisis, so that between 1974 and 1976, each school cohort faced worse economic conditions at labour-market entry than the previous one. Our identification strategy relies on the comparison of very similar pupils – born the same year and with a similar quantity of education (in months) – whose school-leaving behaviour in different economic conditions was exogeneouly implied by compulsory schooling laws. Unlike school-leavers who did postpone their entry on the labour market during the 1980s and 1990s recessions, we provide evidence that pupils' decisions to leave school at compulsory age immediately after the 1973 oil crisis were not endogeneous to the contemporaneous economic conditions at labour market entry. We use a repeated cross section of individuals over 1983-2001 from the General Household Survey (GHS) and take a lifecourse perspective, from 7 to 26 years after school-leaving. Our results show that poor economic conditions at labour-market entry are particularly damaging to women's health. Women who left school in a bad economy are more likely to report poorer health and to consult a general practitioner over the whole period under study (1983-2001). Additional evidence suggests that they are also more likely to suffer from


This chapter was jointly written with Clémentine Garrouste (Université Paris-Dauphine).

a longstanding illness/disability over the whole period. As for men, the health impact of poor economic conditions at labour-market entry is more mixed, and not robust across all specifications. However, we never find that leaving school in a bad economy is beneficial to their health. Finally, our results show that leaving school in a bad economy does not have a lasting impact on labour-market outcomes from 7 to 26 years after school-leaving, neither for men, nor for women.

## 1.1 Introduction

"Chaque tournant torpide de ce monde engendre des enfants déshérités auxquels rien de ce qui n'a été, ni de ce qui sera, n'appartient." Rainer Maria Rilke, Septième Élégie de Duino.

Recent studies in health economics show that socioeconomic circumstances during infancy and early-childhood years have a bearing on health outcomes and mortality later in life (Almond, 2006; Kesternich et al., 2014; Lindeboom et al., 2010; Van den Berg et al., 2006). There is indeed growing evidence there are critical periods for health – in utero and early infancy, but also during childhood and young adulthood (WHO, 2000). This paper investigates whether leaving full-time education in a bad economy is such a critical period for health, i.e. whether it is detrimental to health in the long-run. This is an important question from a policy perspective, as the youth has suffered disproportionately during the Great Recession (Bell and Blanchflower, 2011). Young cohorts who left full-time education in the late 2000s faced historically high unemployment rates. To the extent that leaving school in a bad economy has a lasting and negative impact on health, this situation will most likely generate important health disparities in the future.

There are some reasons to believe that poor economic conditions at school-leaving[1] lead to lower health in the long-run. First, higher unemployment rates at school-leaving may lead to greater stress and trigger addictive behaviours or mental disorders in the short-run. There is indeed evidence that individuals at a high risk of unemployment are more likely to adopt risky health behaviours and suffer more from depressive symptoms in bad times (Dee, 2001; Dave and Kelly, 2012; Charles and DeCicca, 2008).[2] As a result, health may fall immediately after

[1] We use the phrase "school-leaving" or "leaving school" in this paper to mean leaving full-time education.

[2] There is an important literature on the short-term health effect of contemporaneous economic fluctuations. Most studies consider the whole population and use aggregated data. Quite surprisingly, they point to health

school-leaving. If this initial decrease in health is not compensated over the lifecourse, it will generate lasting health disparities between individuals who left school in a bad economy and their luckier counterparts. A second empirical pattern motivating this study has to do with the fact that poor economic conditions at labour-market entry lead to persistent and negative career effects. Recent evidence in labour economics indeed shows that those who graduate in bad economies suffer from underemployment and are more likely to experience job mismatching since they have fewer jobs from which to choose (Kahn, 2010). For instance, graduating from college in a recession has a large, negative and persistent effect on men's wages in the USA and Canada (Kahn, 2010; Oreopoulos et al., 2012).[3] Poor economic conditions at labour-market entry also have adverse effects on men's probability of being employed, especially among the low-educated – although this negative effect generally fades out over the next few years (Genda et al., 2010; Stevens, 2007; Gaini et al., 2012). Workers who enter firms in economic downturns may initially be placed in lower-level jobs with less important tasks and less promotions (Gibbons and Waldman, 2006), so that graduating in a recession may have negative effects on various dimensions of job quality e.g. job stress, perceived job security, working hours, career prospects or more generally working conditions.[4] Overall, there is evidence that adverse economic conditions at graduation have negative consequences on labour-market outcomes – with highly-skilled workers and individuals with a strong attachment to the labour force suffering from larger penalties. As there is both theoretical and empirical evidence that career outcomes are linked to health, one may expect that leaving school in a bad economy has a negative and lasting impact on health through the cumulative impact of these worse career outcomes. Income is indeed generally thought to improve health (Duleep, 1986; Grossman, 1972; Currie, 2009; Gardner and Oswald, 2007), job loss is associated with lower health, adverse health behaviours and higher mortality rates (Sullivan and Von Wachter, 2009; Deb et al., 2011; Salm, 2009; Browning and Heinesen, 2012; Eliason and Storrie, 2009a), while other job dimensions – such as job stress, perceived job insecurity, long working hours, harmful working conditions, downward occupational mobility – have been shown to deteriorate health (Fischer and Sousa-

and health behaviours being countercyclical, at least in the short-run (Buchmueller et al., 2007; Gerdtham and Ruhm, 2006; Neumayer, 2004; Ruhm, 2000, 2003, 2004, 2005). In contrast, recent researchers' findings show that the impact of contemporaneous macroeconomic conditions is highly heterogeneous across worker's ex-ante employment probabilities.

[3]According to Kahn (2010), the catch-up process for wages is as long as 15 years in the US. Similarly, Kondo (2007) finds a negative effect of a recession at labour-market entry on wages in the USA, although the effect is weaker for women than for men.

[4]There is not much work on these aspects to date. A notable exception is Schoar and Zuo (2011), on career prospects. The authors show that economic conditions when CEOs enter the labour market have a long-lasting impact on their career paths and managerial styles – they are less promoted and are in less prestigious occupations than their luckier counterparts.

Poza, 2009b; Fletcher et al., 2011; Caroli and Godard, 2014; Llena-Nozal, 2009; Robone et al., 2011). Overall, these three empirical patterns make a strong case for the study of the long-term health consequences of leaving school in a bad economy.

In this paper, we examine the impact of leaving full-time education in a bad economy on middle and long-term health in England and Wales. We focus on individuals who left full-time education in their last year of compulsory schooling after the 1973 oil crisis. The proportion of pupils who left full-time education at compulsory age in the 1970s was remarkably high in the UK – 50 percent, according to Micklewright et al. (1989). Our identification strategy builds on two sources. First, it relies on the comparison of very similar individuals – born the same year and with a similar quantity of schooling (in months) – whose school-leaving behaviour in different economic conditions was exogeneously induced by compulsory schooling laws. More specifically, within a same birth cohort, pupils born at the end of the calendar year (September to December) were forced to leave school almost a year later than pupils born earlier in the year (January to August). Second, it exploits the sharp increase in unemployment rates generated by the 1973 oil crisis. Between 1974 and 1976, each school cohort indeed faced worse economic conditions at labour-market entry than the previous one.[5] As a consequence, unlucky pupils born in September-December faced higher unemployment rates at labour-market entry than pupils born in January-August of the same calendar year.

Of course, a potential selection issue has to do with the fact that pupils' decisions to leave school at compulsory age may be endogeneous to the contemporaneous economic conditions at labour-market entry. Prior research has indeed linked schooling choice to decreased labour-market opportunities (Gustman and Steinmeier, 1981; Card and Lemieux, 2001; Betts and McFarland, 1995) and shows that individuals tend to remain in school during economic downturns. We show, however, that this is not the case in our setup. Unlike school-leavers who did postpone their entry on the labour market during the 1980s and 1990s recessions, pupils' decisions to leave school at compulsory age between 1974 and 1976 were not endogeneous to the contemporaneous economic conditions at labour-market entry. We argue that the 1973 oil crisis was highly unexpected and that pupils who were in their last year of schooling at that time did not anticipate the adverse career effects of leaving school when unemployment rates were high.

[5] We focus on pupils who left school at compulsory age between 1974 and Easter 1976 – e.g. the 1958 and 1959 birth cohorts. We do not consider older individuals so as to abstract from the effect of the increase in school-leaving age from 15 to 16 from September 1972 on. In our setup, all individuals are affected by the 1972 reform, so that our identification strategy does not rely on the comparison on pre-reform cohorts and post-reform cohorts.

We use a repeated cross section of individuals over 1983-2001 from the General Household Survey (GHS) and take a lifecourse perspective[6], from 7 to 26 years after school-leaving. We investigate the middle to long-term impact of leaving school in a bad economy on health status, health care and health behaviours. We examine the potential labour-market mechanisms by which adverse economic conditions at school-leaving may affect later health. Our results show that poor economic conditions at labour-market entry are particularly damaging to women's health. Women are more likely to report poorer health and have a higher probability of consulting a general practitioner over the whole period (1983-2001). Additional results suggest that they have a higher propensity to suffer from a longstanding illness or disability. As for men, the health impact of poor economic conditions at labour-market entry is more mixed, and not robust across all specifications. Depending of the specification used, our effects range from health-damaging effects to insignificant ones. However, we never find a positive health effect of poor economic conditions at labour-market entry on men's health. Finally, we find that leaving school in a bad economy does not have a lasting impact on labour-market outcomes from 7 to 26 years after school-leaving, neither for men, nor for women.

This paper relates to several strands of literature. First and foremost, it contributes to the emerging literature investigating the long-term health consequences of graduating in a bad economy. To our knowledge, only a very limited number of studies (Maclean, 2013; Hessel and Avendano, 2013; Cutler et al., 2015) have addressed this question. So far, results turn out to be mixed. Maclean (2013) uses US data – the National Longitudinal Survey of Youth 79 (NLSY79) – and exploits the variation in school-leaving state unemployment rates to identify the effect of leaving school in a bad economy on health at age 40. Members of her sample left school between 1976 and 1992. As time or location of school-leaving may be endogeneous to the contemporaneous unemployment rate, she uses instrumental-variable (IV) methods to deal with selection problems related to what she refers to as "endogeneous sorting". She finds that men who left school when the state unemployment rate was high have a higher probability to report poor or fair health as well as depressive symptoms and have lower physical functioning at age 40. Surprisingly, she finds that women leaving school in a bad economy tend to have fewer depressive symptoms at age 40. Hessel and Avendano (2013) use European data, namely the Survey of Health, Ageing and Retirement in Europe (SHARE). They consider individuals

[6]The GHS is a new cross-section in each year so that, although we cannot track any particular individual over time, we can track birth cohorts.

aged 50 and over who left school from 1957 onward. They use country-specific unemployment rates and trend deviations based on the reported year of leaving full-time education. According to their results, poor conditions at school-leaving predict worse health status among women and better health status among men. They provide evidence that highly-educated women are particularly affected. However, the authors acknowledge that both selection into higher education and causation mechanisms may explain this association. Finally, Cutler et al. (2015) use the Eurobarometer data and consider economic fluctuations over 50 years across 31 countries. They show that higher unemployment rates at graduation are associated with lower income, lower life satisfaction, greater obesity, more smoking and drinking later in life, for both men and women. According to their results, education seems to play a protective role, especially when unemployment rates are high. In a series of recent papers Maclean (2014c,a,b) specifically tests whether leaving school in an economic downturn persistently affects drinking behaviour, body weight and the probability of access to an employer-sponsored health insurance. She uses the same methodology and data as in Maclean (2013) and finds that men, but not women, who leave school in a bad economy consume more drinks and are more likely to report heavy and binge drinking than otherwise similar men. Unlucky men have lower bodyweight and are less likely to be overweight and obese at age 40. Finally, she finds that both men and women are less likely to have access to an employer-sponsored health insurance up to 18 years after school-leaving.

Overall, the evidence provided by the literature is rather mixed. Of course, differences in the age groups considered may account for these conflicting results. Differences in terms of labour markets, social security schemes and social policies between the US and Europe may also play a role. In spite of this, additional evidence is needed to understand the long-term health consequences of leaving school in a bad economy – and particularly its heterogeneous impact across gender.

Our paper contributes to the existing literature in several ways. First, our identification strategy is different in spirit from the ones previously used in the literature. Rather than considering long periods of economic fluctuations and exploiting the variation in country (or state) school-leaving unemployment rates, we focus on two birth cohorts only – the 1958 and 1959 cohorts. Our strategy relies on the comparison of similar individuals – born the same year and with a similar quantity of education – whose school-leaving behaviour in different economic conditions was exogeneously induced by compulsory schooling laws. As a consequence, our results cannot possibly be biased by country-specific (or state-specific) cohort effects. Second, we show that pupils' decisions to leave school at compulsory age between 1974 and 1976 were not endogeneous to the contemporaneous economic conditions at labour-market entry – unlike school-leavers during the 1980s and 1990s recessions. There is no need, then, to deal with problems related to endogeneous sorting of school-leaving and our results do not rely on the usual assumptions when implementing instrumental-variables models. Third, our data allow us to adopt a lifecourse perspective, which is only present in the paper by Cutler et al. (2015). Finally, we focus on low-educated individuals. There are some good reasons to focus on pupils leaving school at compulsory age : first, they represent a sizeable proportion of pupils in England and Wales in the mid-70s (approximately 50%). Second, whether they should be more affected than highly-educated individuals by high unemployment rates at labour-market entry – i.e. whether education plays a protective role – remains an open question. On the one hand, economic theory predicts less persistence of poor economic conditions at school-leaving for low-skilled workers and those with weak attachment to the labour force. On the other hand, education has been hypothesised to increase one's ability to cope with negative shocks and uncertainty (Cutler et al., 2015; Cutler and Lleras-Muney, 2006; Smith, 2004). If, according to Cutler et al. (2015), education does play a protective role, leaving school at compulsory age in a bad economy will act as a double whammy. Individuals who leave school early typically have worse health statuses, and more rapidly declining health statuses over the lifecourse than higher-educated ones. If they are disproportionately affected by poor economic conditions at labour-market entry, this will further exacerbate health disparities among education groups. In this context, investigating the long-term health impact of leaving school in a bad economy among low-educated individuals seems crucial.

The rest of the paper is organised as follows. Section 1.2 sketches an economic model of the link between poor economic conditions at labour-market entry and long-term health. Sections 1.3 presents the institutional framework and Section 1.4 the empirical approach. Section 3.3. describes the data that we use. Section 1.5 reports our results and Section 1.6 concludes.

## 1.2 An economic model

In this section, we propose an economic model of the link between economic conditions at school-leaving and health in the long-run. Our model is an extension of the Grossman model of the demand for health (Grossman, 1972). More specifically, it relies on the innovative conceptual framework developed by Galama and Van Kippersluis (2010) in which multiple mechanisms and their cumulative long-term effects can be studied in a structural model of socioeconomic status and health over the life cycle.

Following the usual formulation, health is treated as a form of health capital and individuals derive both consumption (health provides utility) and production benefits (health increases earnings) from it. Health is modeled as a stock that deteriorates over the lifespan and its deterioration can be counteracted by health investment in curative and/or preventive care. Individuals maximize their life-time utility functions :

$$\int_0^T U(t)e^{-\beta t}dt \tag{1.1}$$

where T denotes the life span and $\beta$ is a subjective discount factor. Individuals derive utility $U(t) = U[C_h(t), C_u(t), H(t)]$, where $C_h(t)$ denotes healthy consumption (e.g. healthy food, healthy neighborhood), $C_u(t)$ unhealthy consumption (e.g. smoking or drinking) and $H(t)$ health status. In our framework, time $t$ is measured from the time an individual completes her education and joins the labour force. Utility increases with healthy consumption ($\frac{\partial U(t)}{\partial C_h(t)} \geq 0$), unhealthy consumption ($\frac{\partial U(t)}{\partial C_u(t)} \geq 0$) and with health ($\frac{\partial U(t)}{\partial H(t)} \geq 0$). Individuals maximise their life-time utilities given a budget and a time constraint, and health is defined as :

$$H(t) = I_m(t)^\alpha + (1 - d(t))H(t-1) \tag{1.2}$$

where $H(0)$ and $H(T)$ are respectively the initial and end conditions. Health can be improved through investment in curative medical care $I_m(t)$ and deteriorates at rate $d(t) = d[t, C_h(t), C_u(t), z(t), I_p(t); \xi(t)]$. The health production function $I_m(t)^\alpha$ is assume to exhibit decreasing-returns-to-scale ($0 < \alpha < 1$). $d(t)$ depends on healthy consumption $C_h(t)$, unhealthy consumption $C_u(t)$, "job-related health stress" $z(t)$ (which is interpreted broadly as all physical working conditions and psychological aspects of work that can be harmful to health), investment in curative care $I_p(t)$ and on a vector of exogenous functions $\xi(t)$.

Consumption can be healthy ($\frac{\partial d(t)}{\partial C_h(t)} \leq 0$) or unhealthy ($\frac{\partial d(t)}{\partial C_u(t)} > 0$). Preventive care is modeled analogous to curative care as an activity that provides no utility ($\frac{\partial U(t)}{\partial I_p(t)} = 0$) but is demanded for its health benefits ($\frac{\partial d(t)}{\partial I_p(t)} < 0$). Greater job-related health stress $z(t)$ accelerates the "ageing" process ($\frac{\partial d(t)}{\partial z(t)} > 0$).

In this framework, poor economic conditions at school-leaving can affect health through two distinct – although not mutually exclusive – mechanisms :

1. **An "initial shock effect"**. In this scenario, higher unemployment rates at school-leaving lead to greater stress. This triggers addictive behaviours as well as mental disorders in the short-run, so that health falls immediately after school-leaving. This decrease in health causes the desired level of medical care to rise, but not necessarily enough to restore the health status at the counterfactual level – i.e. the health status of luckier individuals.
   To be more specific, let us consider two identical individuals $a$ (who left school in a bad economy, i.e who is "treated") and $c$ ("non-treated") that differ only in their health at school-leaving. Individual $c$ is supposed to be in better health than individual $a$ ($H^c > H^a$) at school-leaving but is otherwise identical to individual $a$. A smaller health status ($H^a < H^c$) results in a higher optimal level of investment in curative care $I_m^a > I_m^c$. Following Galama and Van Kippersluis (2010), two scenarios can be considered. In scenario 1, the elasticity of health investment with respect to health is assumed to be small. In this scenario, the gap between individuals $a$ and $c$ tends to widen over time.[7] In scenario 2, the elasticity of health investment with respect to health is assumed to be high. In this case, the gap between individuals $a$ and $c$ is likely to close as individuals age.[8]

2. **A "cumulative effect"**. In this scenario (3), we do not assume an "initial shock effect" at school-leaving. Rather, we assume that leaving school in a bad economy has a negative and lasting impact on health through the cumulative impact of worse career outcomes – and in particular through the effect of lower life-time earnings. In our framework, differences between individuals in life-time earnings operate similar to an increase in endowed wealth.[9] Wealthier individuals invest more in curative and preventive care, and their level of healthy consumption is higher. They also engage in work that is more conducive to health, i.e. jobs associated with lower levels of job-related health stress. Overall, higher life-time earnings

[7] In this scenario, individual $a$ tends to consume less healthy consumption $C_h(t)$ and invests less in preventive care $I_p(t)$, while the effect on unhealthy consumption $C_u(t)$ and job-related health stress is ambiguous.

[8] In this scenario, individual $a$ consumes less unhealthy consumption $C_u(t)$, engages less in job-related health stress $z(t)$, and invests more in preventive care $I_p(t)$, while the effect on healthy consumption $C_h(t)$ is ambiguous.

[9] There are indeed reasons to believe that the life-time wealth effect dominates the effect of the increased opportunity cost of time due to higher current earnings (Galama and Van Kippersluis, 2010).

protect health by encouraging healthy life styles and enabling individuals to work and live in healthy environments. In this scenario, poor economic conditions at school-leaving lead to lower health in the long-run through the cumulative impact of lower earnings.

Figure 1.1 presents the evolution of health during the lifespan. The red dashed curves show the potential scenarios for treated individuals, whereas the black solid curve presents the evolution of health for those who are untreated. The blue vertical line stands for the entry on the labour market. The "initial shock" hypothesis is consistent with the idea that health status among treated individuals falls in the short-term (scenarios 1 and 2). In scenario 1, the desired level of medical care rises in order to restore health, but not enough to restore the counterfactual level of health in the long-run. In scenario 2, this level rises as its counterfactual level, so that no health disparities are observed in the long-run. The "cumulative effect" hypothesis is depicted by scenario 3. In this scenario, differences in life-time earnings lead to a widening health gap between treated and non-treated individuals. Note that this health gap may be persistent in the long-run even if earnings among treated and non-treated individuals finally catch up at some point.

## 1.3 Institutional framework

This section describes the compulsory schooling laws in England and Wales (see section 1.3.1) and provides graphical evidence of the sharp increase in unemployment rates after the 1973 oil crisis (see section 1.3.2).

### 1.3.1 Compulsory schooling in England and Wales

The British compulsory schooling laws specify the maximum age at which pupils have to start school and the minimum age at which pupils are allowed to leave school.

The official school-starting age is the beginning of the term starting after the child's fifth birthday. Hence, entry rules determine that a school cohort consists of children born between the first day of September and the last day of August in the following calendar year (Del Bono and Galinda-Rueda, 2007). In other words, due to the discontinuity introduced by the school-entry rule, students within a same birth cohort belong to different school cohorts. There is evidence that compliance with school-entry requirement is almost perfect and that grade repetition (or grade skipping) is almost non-existent in England and Wales (Sharp et al., 2002; Grenet, 2013).

The current school leaving age of 16 was increased twice in England and Wales[10], from age 14 to 15 in 1947 and from age 15 to 16 in 1972.[11] The proportion of children leaving education at the first legal opportunity in the UK is high by the standards of other industrialised countries (Micklewright et al., 1989). In the early 1960s, only about 20% of pupils stayed in full-time education after having reached the minimum school-leaving age (Del Bono and Galinda-Rueda, 2007; McVicar and Rice, 2001). In our data, this proportion amounts to 50% in the mid-1970s. After the 1972 Raising Of the School-Leaving Age (ROSLA), students in their last year of compulsory schooling were normally attending secondary school (Year 11) while the less academically inclined were in vocational training. Two types of qualifications could be obtained at the end of Year 11 : the General Certificate of Education Ordinary Level (GCE O level) or the Certificate of Secondary Education (CSE). Both credentials were awarded at the end of junior secondary school, after an examination (Grenet, 2013).
Unlike other countries – and unlike the USA –, the implementation of compulsory schooling in England and Wales differs in that a student is not allowed to leave school on the exact date (birthday) in which she reaches the school-leaving age. Between school years 1963-1964 and 1996-1997, (see the Education Act of 1962, Appendix A-1.3.), the rules governing school exit implied that pupils who reached age 16 between the $1^{st}$ of September and the $31^{st}$ of January had to complete their education until the following Easter. Students who reached the age of 16 between the $1^{st}$ of February and the end of August were forced to leave school at the end of the summer term, typically in May/June. Pupils born between the end of the summer term and August – i.e. pupils born in July or August – were thus allowed to leave school before their $16^{th}$ birthday, i.e. at age 15.

To show how these exit rules support our identification strategy, we present in Figure 1.2 the authorised school-leaving date with respect to students' month-year of birth. It makes it clear that students born in the same calendar year belonged to different school cohorts due to the discontinuity introduced by the school entry rule (see column 3). It also provides evidence that, within the same birth cohort, the oldest pupils – born between January and August – were allowed to leave school at Easter or in May/June of year $t$ whereas the youngest – born

[10] The education system in Scotland is different and not considered here.

[11] Several studies use these changes in minimum school-leaving age to identify the returns to education on labour market outcomes and health (Harmon and Walker, 1995; Oreopoulos, 2006; Devereux and Hart, 2010; Grenet, 2013; Clark and Royer, 2013). Note that in our setup, however, all individuals are affected by the 1972 ROSLA reform. Our identification strategy does not rely on the comparison on pre-reform cohorts and post-reform cohorts.

between September and December – were not allowed to leave school until the following Easter of year *t+1*. Figure 1.3 provides an illustration of how the compulsory schooling rules operate by taking the 1958 birth cohort as an example. Note that, due to the discontinuities introduced by both school-entry and school-exit rules, pupils born in different months had a similar quantity of schooling (in months) at the end of full-time education. A maximum difference of three months of education upon reaching the final year of schooling was induced by the existence of two specific school-leaving dates (Easter or the end of the summer term). It is highly unlikely, however, that this three-month difference should have an impact on health. Clark and Royer (2013) indeed show that the additional year of schooling induced by the 1972 ROSLA reform had no effect on health whatsoever. In this context, it seems highly unlikely that a three-month difference in compulsory schooling may have a determinant impact on health – especially as the pupils considered by Clark and Royer (2013) are very similar to the pupils considered here.

### 1.3.2 Unemployment rates

The sharp and unprecedented increase in the oil price from three to ten dollars a barrel in October 1973 had serious effects on the balance of payments of the industrial nations, which were oil-importer countries. This first world-wide recession had strong effects on unemployment rates in a number of industrialised countries, including the UK (Bhattarai, 2011).[12]

Figure 1.4 provides evidence of the sharp increase in unemployment rates after the 1973 oil crisis. The 1973 oil crisis – which occurred in October 1973 – is symbolised by the vertical dark blue line on the left-hand side. The blue line shows the unemployment rates for all individuals aged 16-64[13] and the red and green lines show the unemployment rates for men and women respectively. As shown in Figure 1.4, unemployment rates gradually increased between 1974 and 1978 – when the economy recovered – with the sharpest increase between 1974 and 1976. The vertical blue areas on Figure 1.4 indicate the periods at which each school cohort was allowed to leave school, i.e. at Easter/May/June. As made clear by the graph, each school cohort faced significantly higher unemployment rates than the previous school cohort.[14]

[12]Thus, it can reasonably be argued that the 1973 crisis was not endogenous to health in the UK.

[13]Unemployment rates (UR) are provided by the Office for National Statistics (ONS). UR for individuals aged 16-25 are not available on a monthly basis from the ONS for the period under study. We compute UR for the 16-25 on an annual basis using the 1975 and 1977 waves of the UK Labour Force Survey (LFS). Our computations show that UR among the 16-25 were high, and increased from 7.34% in 1975 to 9.05% in 1977, corresponding to a 23% increase within a two-year period. This increase lies in the same range of magnitude as the increase in UR experienced by individuals aged 16-64 – from 4.5% in 1975 to 5.6% in 1977, corresponding to a 24% increase.

[14]The unemployment rate increased by 0.7 percentage point between Easter/May/June 1974 and Easter/May/June 1975. It increased by 1.1 percentage point between Easter/May/June 1975 and

## 1.4 Empirical approach

Section 1.4.1 presents our main identification strategy as well as the model we estimate. Section 1.4.2 discusses the validity of this identification strategy and presents a placebo test. Section 1.4.3 introduces a differences-in-differences strategy as an additional specification.

### 1.4.1 Identification strategy and model

We consider pupils who left school at minimum school-leaving age and who entered the labour market between Easter 1974 and Easter 1976, i.e. the 1958 and 1959 birth cohorts. We do not consider older individuals so as to abstract from the effect of the increase in school-leaving age from 15 to 16 from September 1972 on.

Our identification strategy relies on the comparison of similar individuals – born the same year and with a similar amount of education (in months) – whose school-leaving behaviour in different economic conditions was exogeneously induced by compulsory schooling laws (both school entry and exit rules). More specifically, within a same birth cohort, pupils born at the end of the calendar year (September to December) were forced to leave school almost a year later than pupils born earlier in the year (January to August). We exploit the fact that between 1974 and 1976, each school cohort faced worse economic conditions at labour market entry than the previous one (due to the sharp increase in unemployment rates generated by the 1973 oil crisis). Thus, within each birth cohort, pupils born between September and December faced higher unemployment rates at labour-market entry than pupils born between January and August. Note that our identification strategy does not rely on the comparison on individuals who left school *before* and *after* 1973.[15] In our setup, all individuals are affected by the 1973 oil crisis. However, some pupils (the "treated") left school in worse conditions than otherwise similar pupils.[16]

Easter/May/June 1976. This increase was somewhat milder between 1976 and 1977 as well as between 1977 and 1978 (a 0.1 percentage point increase in both cases).

[15] This is because pupils who left school at minimum school-leaving age before and after the 1973 oil crisis are not comparable. Those who left school at compulsory age *before* the crisis were 15 years old, while those who left school at compulsory age *after* the crisis were 16 years old (due to the 1972 ROSLA reform). This difference in years of education makes it difficult to attribute the differences in health outcomes to economic conditions at labour-market entry.

[16] The 1973 oil crisis had a disproportionate impact on some regions – typically in Wales and the North of England. However, we do not use this additional regional variation. First, we do not have reliable data on UK regional unemployment rates at a sufficiently disaggregated level in the 1970s. Second, we do not have information on the actual region in which the individual lived at age 16.

We use a repeated cross-section of individuals over 1983-2001 to estimate the following equation by standard probit, for men and women separately :

$$H_i^* = \alpha + \gamma T_i + BirthYear_i + f(BirthMonth_i) + InterviewYear_i + \epsilon_i \tag{1.3}$$

where $H_i^*$ denotes the latent health status of individual $i$ and is only observed as:

$$H_i = \mathbb{1}_{\{H_i^*>0\}} \tag{1.4}$$

and where $T_i$ is a dummy variable taking value 1 if individual $i$ is treated, i.e. born between the $1^{st}$ of September and the $31^{st}$ of December and value 0 if non-treated, i.e. born between the $1^{st}$ of January and the $31^{st}$ of August. $BirthYear_i$ is a dummy variable for individual $i$'s year of birth. $InterviewYear_i$ is a dummy variable for individual $i$'s interview year.[17] $f(BirthMonth_i)$ is a linear function of age in months within a birth year. We define it as $(12 - BirthMonth_i)$, where $BirthMonth_i$ denotes the month of birth of respondent $i$ and varies from 1 to 12.[18] We include this linear function of age in Equation (3.3) to account for the fact that within each birth cohort, treated individuals (born September-December) are younger than non-treated pupils (born January-August). As age and health are negatively correlated, not taking into account this age difference – which is a difference in months within a birth cohort – may lead us to underestimate the negative impact of leaving school in a bad economy.[19] Finally, $\epsilon_i$ denotes the error term.

Equation (3.3) estimates the average effect of leaving school in a bad economy on health over the whole period ($\widehat{\gamma}$). But our empirical approach also allows us to take a lifecourse perspective. To do so, we compute the marginal effects of the treatment associated with each interview year

[17] We control for *InterviewYear* to account for the fact that we observe individuals at different points in time. We choose to include a dummy indicating the year in which an individual is interviewed rather a dummy indicating the survey wave in which she is interviewed. This is because a survey wave can be conducted over several years – usually two.

[18] One may worry that introducing simultaneously the variables $T_i$, $BirthYear_i$ and $(12 - BirthMonth_i)$ in Equation (3.3) should lead to multicollinearity issues. When estimating Equation (3.3), we find that the VIF (Variance Inflation Factor) criterion is lower than 10 for all variables, suggesting inconsequential multicollinearity (see the rule of thumb provided by Hair et al. (1995)).

[19] As expected, estimating Equation (3.3) without the linear function of age in months yields very similar estimates, although of lower magnitude and less significant (results are not shown but available upon request).

over 1983-2001.[20] This allows us to investigate whether the impact of poor economic conditions at labour-market entry on health is driven by middle or long-term effects.

A key assumption is that pupils in their last year of compulsory schooling do not stay strategically in school when the economy deteriorates, i.e. do not engage in what we refer to as "endogeneous timing". If pupils anticipate the adverse effects of leaving school in a bad economy and enroll in an additional year of schooling, our estimates will be biased. We discuss this point in section 1.4.2.1. A second identifying assumption is that if there are no other institutional differences within each birth cohort generating differences in health among the treated and the non-treated apart from school-exit rules (see section 1.4.2.2 for a discussion on school-entry rules and section 1.4.2.3 for a discussion on the differential incentives to take GCE O-level/CSE examinations induced by the January/February discontinuity), we can safely attribute observed differences in health to the impact of labour-market conditions at labour-market entry. To the extent that individuals born between January and August and individuals born between September and December are identical in all observable and unobservable characteristics (see section 1.4.2.4 for a discussion of the potential effects of season of birth) the differences in health status will be driven only by school-exit rules and hence different unemployment rates at labour-market entry, thus allowing us to identify the health consequences of leaving school in a bad economy.

### 1.4.2 Validity of the identification strategy

#### 1.4.2.1 Endogenous timing of school-leaving

Time of school-leaving may be endogeneous to the contemporaneous economic conditions. The sign of the bias arising from endogeneous timing, however, is difficult to predict. On the one hand, school-leavers who avoid leaving school in a bad economy may have unobserved characteristics (e.g. financial resources, other parental characteristics) that allow them to postpone their entry on the labour market. On the other hand, it is likely that only the most capable and hardworking are able to leave school during a bad economy since their abilities allow them to secure desirable jobs regardless of the economic conditions. These characteristics may be

[20] More specifically, we estimate Equation (3.3) and substitute the interaction term $T_i * InterviewYear_i$ for $T_i$. Interview-year specific marginal effects correspond to the estimated marginal effects associated with the interaction terms.

correlated with subsequent health, in which case our estimates will be biased.

Whether pupils in their last year of compulsory schooling stay strategically in school when the economy deteriorates is an empirical question. For each birth cohort, Figure 1.5 shows the proportion of pupils who left school at compulsory age among the treated and non-treated group. It also pictures the one-year growth in school-leaving unemployment rates (calculated for the March-June period) faced by the youngest school cohort (treated) – as compared to the previous school cohort (non-treated). When considering the 1958 and 1959 birth cohorts, Figure 1.5 shows that within each birth cohort, the proportion of pupils who left school at compulsory age among the treated and the non-treated group is equal, indicating that school-leaving behaviour in last year of compulsory schooling was not shaped by the sharp increase in unemployment rates generated by the 1973 oil crisis. Although treated pupils from the 1958 (1959) birth cohort faced a 21% (resp. 23%) increase in unemployment rates as compared to luckier pupils born January-August, they did not enroll in an additional year of schooling.[21] When considering younger birth cohorts, however, we do find that a sharp growth in unemployment rate (e.g. the 1980s and 1990s recessions) is associated with a significant decrease in the proportion of treated pupils leaving school at compulsory age.[22]

Overall, we find no evidence that school-leavers born in 1958-1959 – the cohorts that we consider – did engage in endogeneous timing of school-leaving. When considering younger birth cohorts, however, we do find that a sharp growth in unemployment rate (e.g the 1980s and 1990s recessions) is associated with a decrease in the proportion of treated pupils leaving school at age 16. It can be hypothesised that pupils in their last year of compulsory schooling in 1974-1976 did not anticipate the adverse consequences of high unemployment rates at labour market entry – contrary to school-leavers in the 1980s and 1990s recessions. It may be due to the fact that the 1973 oil crisis was highly unexpected and was the first post-war crisis to generate such a sharp increase in unemployment rates.

[21] One may argue that even if the *proportion* of pupils who left school at compulsory age is equal in the treated and non-treated group, the *composition* of each group might be different. Due to the lack of information on individual characteristics at age 16, we cannot test this assumption in a proper way. However, we use information on father's occupation and show that among pupils born the same year who left school at compulsory age, the proportion of pupils whose father was in a manual occupation was equal whether they were treated or not.

[22] The two proportions are significantly different for the 1963-1964 and 1973-1974 birth cohorts. Interestingly, this result seems to suggest that rather than high unemployment rates or even increasing unemployment rates, it is a sharp increase in unemployment rates – typically occurring during recessions – that induces endogeneous timing among pupils in their last year of education.

#### 1.4.2.2 School-entry rules

School-entry rules introduce a discontinuity between August-born and September-born children. This discontinuity implies that students within a same birth cohort belong to different school cohorts. This institutional feature may generate health differences within a same birth cohort between treated and untreated pupils by means of age-relative rank, school-cohort size or job-experience effects. We discuss these issues in what follows.

*Age-relative rank*

School-entry rules imply that treated individuals (born September-December) are the oldest pupils in their *school cohort*, while untreated pupils are the youngest.[23] Yet, there is evidence that relative age effects play a role in school performance. More specifically, older people in a given school cohort tend to have higher wages than younger individuals in the same school cohort – which is interpreted as an indication of the persistence of maturity effects related to age differences between students in the same class (Plug, 2001). As treated pupils are the oldest in their school cohort – and to the extent that relative maturity effects positively affect adult labour market and health outcomes – we should measure a lower bound, i.e. underestimate the negative impact of poor economic conditions at labour market entry on long-term labour market and health outcomes.

*School-cohort-size effects*

Since treated and non-treated pupils belong to different school cohorts, another concern has to do with school-cohort sizes. School-cohort size has been shown to have a negative impact on labour market outcomes due to an excess of supply on the labour market (Welch, 1979; Berger, 1985, 1989; Macunovich, 1999; Korenman and Neumark, 1997; Morin, 2011). We focus on three school cohorts only, which are not likely to be different in size.[24] To the extent that cohort-size effects exist, however, we should measure a lower bound : the fertility rate peaked in 1957 and declined after that, so that for a given birth year the youngest school cohort (treated) would have higher wages on average than the previous school cohort.

[23] Conversely, treated individuals (born September-December) are the youngest pupils in their *birth cohort*, while untreated pupils are the oldest.

[24] Cohort sizes do not vary substantially from one year to the next. This is why studies have focused on long-term (typically 8-25 years) variations in cohort size (Morin (2011)).

*Job-experience effects*

School-entry rules imply that within a birth cohort, treated pupils start school one year later than non-treated pupils. As starting school later entails the opportunity cost of entering the labour market later, treated pupils lack one year of job experience as compared to non-treated pupils. However, whether an additional year of job experience has a positive or negative impact on health is not clear. On the one hand, it leads to higher life-time earnings, which is beneficial to health. On the other hand, it implies a longer exposure to adverse working conditions, if any. To the extent that we consider low-skilled individuals, this possibility is not to be discarded. Hence, the direction of the effect of an additional year of job experience on health is not clear.

#### 1.4.2.3 Differential incentives to take GCE O-level/CSE examinations

Depending on their date of birth (before or after January $31^{st}$), individuals within a same school cohort were allowed to leave school only after one of two specific dates (Easter or the end of the summer term) upon reaching their final year of schooling. Pupils who left school at the end of the summer term, however, had higher incentives to take the exam at the end of Year 11 (O-Level/CSE qualifications) in which they could be awarded nationally-recognized qualifications.[25] In this context, the January/February discontinuity might introduce a bias in our analysis : treated pupils (born between September and December) are allowed to leave at Easter, and have less incentives to take the exam at the end of the year. It might impact their educational achievement as well as their adult labour market and health outcomes.
We check in the robustness section that this differential incentive in taking the exams at the end of Year 11 is not likely to bias our results.

#### 1.4.2.4 Season-of-birth effects

Our identification strategy assumes that individuals born between January and August and individuals born between September and December are identical in all observable and unobservable characteristics. Yet, a growing body of literature has shown the importance of season-of-birth effects on subsequent labour and health outcomes (Bound and Jaeger, 1996).

[25] Del Bono and Galinda-Rueda (2007) exploit this January/February discontinuity in a regression discontinuity design and estimate the impact of three additional months of compulsory schooling on educational attainment and longer labour market outcomes. In this paper, we do not exploit this January/February discontinuity for two reasons : (i) unemployment rates do not vary enough between Easter and the end of the summer term and (ii) our sample would probably be too small to detect any effect.

First, the seasonality of births varies from one social group to another. On US data, Kestenbaum (1987) reports that children born to high-income families are more likely to be born in spring.[26] In our framework, it implies that untreated pupils should have more favourable parental socio-demographic characteristics. To the extent that children born to high-income families are in better health on average, this would lead us to overestimate the impact of poor economic conditions at labour-market entry on adult health outcomes. Beyond parental socio-economic characteristics, some health differences have been proved to show dependence with respect to birth date, too (Bound and Jaeger, 1996). Doblhammer and Vaupel (2001) have shown a positive relationship between being born in October to December and longevity at age 50.[27] This month-of-birth effect suggests that even in the presence of parental socio-demographic characteristics, treated pupils should be in better health than untreated pupils, which would lead us to underestimate the impact of economic conditions at entry on adult health outcomes.

#### 1.4.2.5 Placebo test

Overall, only job-experience effects should lead us to overestimate the negative health impact of poor economic conditions at labour-market entry. As a first step, we check that our estimates are not upward-biased due to job-experience effects by running a placebo test on the 1953-1954 birth cohorts. The 1953-54 birth cohorts faced very similar school-leaving unemployment rates at the end of compulsory schooling. Moreover, the same schooling rules applied for these cohorts (see the 1962 Education Act, Appendix A-1.3.), except that the minimum school-leaving age was then 15. School-leaving unemployment rates (averaged over March-June) varied from 2.475 to 2.675 over a three-year period (1968-1970).[28] Importantly, all pupils born in 1953-1954 who left school at compulsory age did so after the major events of 1968.

[26] Note, however, that we do not find evidence of this in our data. When considering the whole GHS sample and using information on father's occupation (manual or not), we find that the proportion of individuals whose father was in a manual occupation was the same whether individuals were born in September-December or earlier in the year.

[27] They show that those born in the northern hemisphere in October to December live about as much as 0.6 year longer than those born in April to June. As expected, data for Australia show that, in the Southern Hemisphere, the pattern is shifted by half a year. They conclude that the month-of-birth effect is most likely explained by the seasonal availability of fresh fruit, vegetables and eggs to the pregnant mother in the first and second trimesters.

[28] Unemployment rates from the Labour Force Survey (LFS) are not available prior to 1973. We use instead unemployment rates from administrative data – namely the monthly "registrant count" (borrowed from Denman and McDonald (1996)) – to compute these averages.

### 1.4.3 A differences-in-differences approach

As a second step, we implement a differences-in-differences analysis. This strategy allows us to eliminate any systematic differences between September-December born children and January-August born children (e.g. job experience, season-of-birth, or any other time-invariant characteristic). We use the 1953-1954 cohorts as a "control" group and estimate the following equation by a linear probability model :

$$H_i = \alpha + \chi T_i + \delta D_i + \beta T_i \times D_i + BirthYear_i + f(BirthMonth_i) + InterviewYear_i + \epsilon_i \quad (1.5)$$

where $D_i$ is an indicator variable taking value 1 if individual $i$ is born in 1958-1959 and value 0 if born in 1953-1954. $\widehat{\beta}$ is the differences-in-differences estimator. It corresponds to the difference in health between the treated and untreated individuals across the 1958-59 and 1953-54 cohorts. We assume that if the treated had not been subjected to the treatment (i.e. an increase in unemployment rates at school-leaving as compared to the previous school cohort), both treated and untreated groups would have experienced the same trend in health (Lechner, 2010).

## 1.5 Results

### 1.5.1 The impact of leaving school in a bad economy on health

In this section, we successively present our main results (see section 1.4.1), the placebo test (see section 1.4.2.5) as well as the results obtained when implementing the differences-in-differences approach (see section 1.4.3).

#### 1.5.1.1 Main results

Probit estimates of Equation (3.3) are presented in Table 1.4 for men and women respectively. Each line presents the marginal effect (resp. standard error and number of observations used in the model) of having left school in a bad economy (i.e. being treated) for a different health outcome. All our models include dummy variables for interview and birth years as well as a linear function of age – see Equation (3.3). Our results show that over the whole period (1983-2001), men who left school in a bad economy face a 17 percentage-point increase in the probability of having ever smoked (at the 5% significance level). As regards the other health outcomes, the marginal effects for men do not appear to be statistically significant at conventional levels. Leaving school in a bad economy, however, seems to be particulary health-damaging for women. Marginal effects in Table 1.4 imply that women who left school in a bad economy have a 11 percentage-point higher probability of reporting poor self-rated health (at the 10% significance level) over the whole period (1983-2001). Consistently, women are also more likely to consult a GP during the last two weeks (a 12 percentage-point probability increase, at the 5% significance level) over the whole period. In contrast, leaving school in a bad economy does not seem to affect women's propensity to restrict their activities due to illness or injury, to suffer from a longstanding illness/disability, nor to go to the hospital during the 12 months preceding the interview. It does not seem to be particularly harmful to women's health behaviours such as smoking and drinking, either.

Figures 1.6 to 1.8 present the impact of having left school in a bad economy on health outcomes in a lifecourse perspective. While estimates in Table 1.4 provide the average impact of poor economic conditions at labour-market entry over the whole period (1983-2001), Figures 1.6 to 1.8 allow investigating whether this impact is driven by middle or long-term effects. Each figure pictures interview-year specific marginal effects over 1983-2000 of having left school in a bad economy (i.e. of being treated) for a different health outcome, for men and women separately. For the sake of conciseness, these figures are only presented for health outcomes previously found to be significant in Table 1.4. For instance, Figure 1.6 shows the interview-year specific marginal effects of poor economic conditions at labour-market entry on the probability of having ever smoked for men. Correspondingly, Figure 1.7 (resp. Figure 1.8) shows the marginal effects of poor economic conditions at labour-market entry on the probability of reporting poorer health (resp. consulting a GP) for women.

Overall, these figures show that the average impact of leaving school in a bad economy on health does not seem to be particularly driven by middle or long-term effects – for each figure, the majority of marginal effects lie above the zero line for all interview years. This suggests that men's smoking behaviour as well as women's health seem to be negatively affected by poor economic conditions at labour-market entry over the whole period under study.

#### 1.5.1.2 Placebo test

We investigate to which extent being born between January and August (as compared to being born between Semptember and December) influences health and labour outcomes not in terms of economic conditions at labour-market entry but by means of unobservable characteristics (age relative rank, season-of-birth effects etc.). As a first step, we re-run our probit models on the 1953-1954 cohorts. Results are presented in Table 1.5. As expected, we find no significant effect of being born between January and August – as compared to being born between September and December – on any health outcome. All coefficients are insignificant at conventional levels.

#### 1.5.1.3 A differences-in-differences approach

The placebo test has provided first evidence that our main results were not likely to be biased by any systematic (unobservable) differences between September-December and January-August born children. To further investigate this matter, we implement a differences-in-differences (DiD) strategy. This strategy uses the 1953-54 cohorts as a "control" group. It controls for any systematic differences between September-December born children and January-August born children. Linear probability estimates of Equation (1.5) are presented in Table 1.6 for men and women respectively. Marginal effects in Table 1.6 imply that women who left school in a bad economy face a 6 percentage-point increase in the probability of reporting poor self-rated health (at the 10% significance level) over the whole period (1983-2001). Correspondingly, poor economic conditions at labour-market entry increase by 6 percentage points women's probability of consulting a GP during the last two weeks (at the 5% significance level). When controlling for any systematic differences between September-December and January-August born children, we find that women are also more likely to suffer from a longstanding illness/disability (a 8 percentage-point increase, significant at the 1% level) over the whole period. Overall, the results obtained for women when implementing a DiD strategy confirm our findings from the main analysis. In particular, the DiD estimates are in the same range of magnitude as those presented in Table 1.4. Our main results for men, however, are not robust to the DiD specification. Results from Table 1.6 show that the effect of poor economic conditions at labour-market entry on men's smoking behaviour is no longer significant.

Overall, our findings when implementing the DiD strategy make us confident that our main estimates for women capture the true effect of poor economic conditions at labour-market entry

– as opposed to any systematic differences between September-December and January-August born children.

### 1.5.2 The impact of leaving school on labour-market outcomes

Labour market characteristics can be viewed as mechanisms by which leaving school in a bad economy affects long-term health. To investigate this, we regress labour market proxies on the treatment variable $T_i$, on year-of-birth and interview dummies as well as on the linear function of age. Models are estimated by OLS or probit – depending on the nature of the dependent variable (continuous or dichotomous)).

Table 1.7 presents the effect of leaving school in a bad economy on labour-market outcomes for men and women respectively. We find no effect on unemployment, inactivity patterns and earnings[29], neither for men, nor for women. While women who left school in a bad economy do not seem to have been in their current job for a shorter period of time, men have a higher probability of being in their current job for less than one month (coeff : 0.074, significant at the 5% level). This is consistent with the idea that poor economic conditions at labour-market entry have a negative effect on job tenure. However, the fact that untreated individuals have an additional year of job experience as compared to treated ones (which is due to the fact that they entered the labour market one year earlier) could also account for this result. This effect should be captured by our DiD estimates, though. When implementing the DiD model on labour-market proxies, the effect on job tenure (i.e. being in a current job for less than one month) for men vanishes, suggesting that our previous result was mostly driven by job-experience effects. Other DiD estimates (not shown) are very similar to the ones presented in Table 1.7.

Overall, we do not find that leaving school in a bad economy has a lasting impact on labour-market outcomes 7 to 26 years after school-leaving. This is not really surprising, though, as we do consider low-educated individuals. Economic theory indeed predicts less persistence of poor economic conditions at school-leaving for low-skilled workers subsequent labour-market outcomes. Stevens (2007), Gaini et al. (2012) and Genda et al. (2010) provide evidence that the negative effect of graduating in a bad economy on labour-market outcomes vanishes after a few years (usually four or five) when considering low-educated individuals in Germany, France and the USA.

[29] Our results hold when estimating Tobit models for earnings (results not shown but available upon request)

### 1.5.3 Robustness Checks

This section performs several robustness checks using our main specification (see Equation (3.3)).

#### 1.5.3.1 Differential incentives to take GCE O-level/CSE examinations

One may worry that treated pupils have fewer incentives to take examinations at the end of Year 11. It might impact their educational achievement and later health outcomes. To control for this potential bias, we re-run our regressions controlling by a dummy variable indicating whether the individual holds a Year-11-equivalent qualification (O-level, CSE etc.). Our results are virtually unchanged.

#### 1.5.3.2 Alternative empirical approach

Up to now, our treatment variable has been a dummy variable indicating whether an individual was born at the end of the calendar year or earlier in the year (see Equation (3.3)). A possible drawback of this approach is that it linearises the impact of the treatment across the two birth cohorts – which may be problematic to the extent that treated pupils do not face the same increase in school-leaving unemployment rates as compared to non-treated pupils across the two birth cohorts (a 0.7 and a 1.1 point increase respectively).

To deal with this potential problem, we estimate the following equation by standard probit :

$$H_i^* = \lambda + \pi UR_i + BirthYear_i + f(BirthMonth_i) + InterviewYear_i + \eta_i \qquad (1.6)$$

where $H_i^*$ denotes the latent health status of individual $i$ and is still observed as a dummy variable. $UR_i$ stands for the school-leaving unemployment rate faced by individual $i$ and the other variables are presented in section 1.4.

Probit estimates of Equation (1.6) are presented in Table A-1.1 and are very similar to the ones presented in the main analysis (see Table 1.4), although less precisely estimated. In particular, estimates in Table A-1.1 imply that a one-point increase in school-leaving unemployment rates leads to a 10 percentage-point increase in the probability of reporting poor health (at the 10% significance level), and a 8 percentage-point increase in the probability of consulting a GP (at the 15% level) among women. As for men, a one-point increase in school-leaving unemployment rates leads to a 14 percentage-point increase in the probability of having ever smoked (although the effect is marginally significant at the 15% level). Using this specification, we also find that men's health is negatively affected by poor economic conditions at labour-market entry : a one percentage-point increase in school-leaving unemployment rates leads to a 13 percentage-point increase in the probability of reporting poor health (at the 15% significance level) and a 7 percentage-point increase in the probability of restricting one's activity due to illness or injury (at the 10% significance level).

#### 1.5.3.3 A "bad economy"?

Until now, we have implicitly considered that leaving school in a bad economy was equivalent to leaving school at a time when unemployment rates sharply increased. But more generally, the term leaving school "in a bad economy" should measure the propensity to suffer from underemployment and to experience job mismatching at labour-market entry. To this extent, unemployment rates may be important, but so may hiring practices or seasonal fluctuations of the labour-market. Hiring practices may play an important role, especially as the 1972 ROSLA reform induced the removal of a whole year's school leavers from the labour market in 1973. To this extent, pupils who left school at minimum school-leaving age in 1974 (untreated) did enter the labour-market at a time when (i) unemployment rates were relatively low (ii) and when employers were eager to hire low-skilled individuals due to the recent shortage. This implies that within the 1958 birth cohort, treated individuals did leave school in a worse economy than untreated ones, which is consistent with our interpretation. Turning to seasonal fluctuations of the labour-market, one may worry that leaving school at Easter (rather than at the end of the summer term) should be beneficial in terms of labour-market outcomes. It may indeed be easier to find a job at Easter rather than during the summer term. In this case, seasonal fluctuations of the labour-market should be beneficial to the treated – who were all allowed to leave at Easter. It would imply that although treated pupils left school a year later than untreated pupils – a period during which the state of the economy deteriorated – this deterioration would be partly offset by the seasonal fluctuations of the labour-market. Figure A-1.1 displays monthly unemployment rates (not seasonally adjusted) over the 1973-1976 period[30] and shows that seasonal labour-

[30]Monthly unemployment rates (UR) come from an administrative source, namely the "monthly registrant count". The registrant count method counts the number of people who registered themselves as unemployed. Note that people who registered themselves as unemployed did not automatically go on to make a claim for unemployment-related benefits, but registration was a prerequisite for entitlement. Note also that during this period, school leavers aged 16 and 17 fell within the eligibility criteria for unemployment benefits. UR calculated from the "monthly registrant count" differ from the UR measured from the Labour Force Survey used previously

market fluctuations between Easter and May/June were not likely to play an important role. It provides evidence that unemployment rates did not follow a particular seasonal pattern between Easter and the end of the summer term (May/June) over the period under study. Overall, it appears that both hiring practices and seasonal fluctuations of the labour market do not invalidate the interpretation of our results, as untreated pupils do seem to enter the labour-market in a bad economy anyway.

## 1.6 Conclusion

In this paper, we investigate the impact of leaving school in a bad economy on long-term health status, health care consumption and health behaviours. We consider pupils in England and Wales who left school in their last year of compulsory schooling immediately after the 1973 oil crisis and whose school-leaving behaviour in worse economic conditions was exogeneously induced by compulsory schooling laws. We provide evidence that these pupils did not engage in endogeneous timing. We use a repeated cross section of individuals over 1983-2001 from the General Household Survey (GHS) and take a lifecourse perspective. We find that poor economic conditions at labour-market entry are particularly damaging to women's health. Women who left school in a bad economy are more likely to report poorer health and to consult the General Practitioner over the whole period under study (1983-2001). Additional evidence suggests that they are also more likely to suffer from a longstanding illness/disability over the whole period. As for men, the health impact of poor economic conditions at labour-market entry is more mixed. Men who left school in a bad economy seem to be negatively affected in various dimensions (smoking status, and to some extent health status), although these effects are not robust to all specifications. This may be due to a power problem, as our sample for men is smaller in size than that of women. Finally, we do not find any significant effects of poor economic conditions at labour-market entry on subsequent labour-market outcomes (from 7 to 26 years after school-leaving), which is consistent with the literature.

The large and lasting health-damaging impact that we find among women raises the issue of the mechanisms through which poor economic conditions at labour-market entry affect long-term health. Our results are consistent with both the "initial shock" and the "cumulative effect"

(which are measured according on the ILO definition).

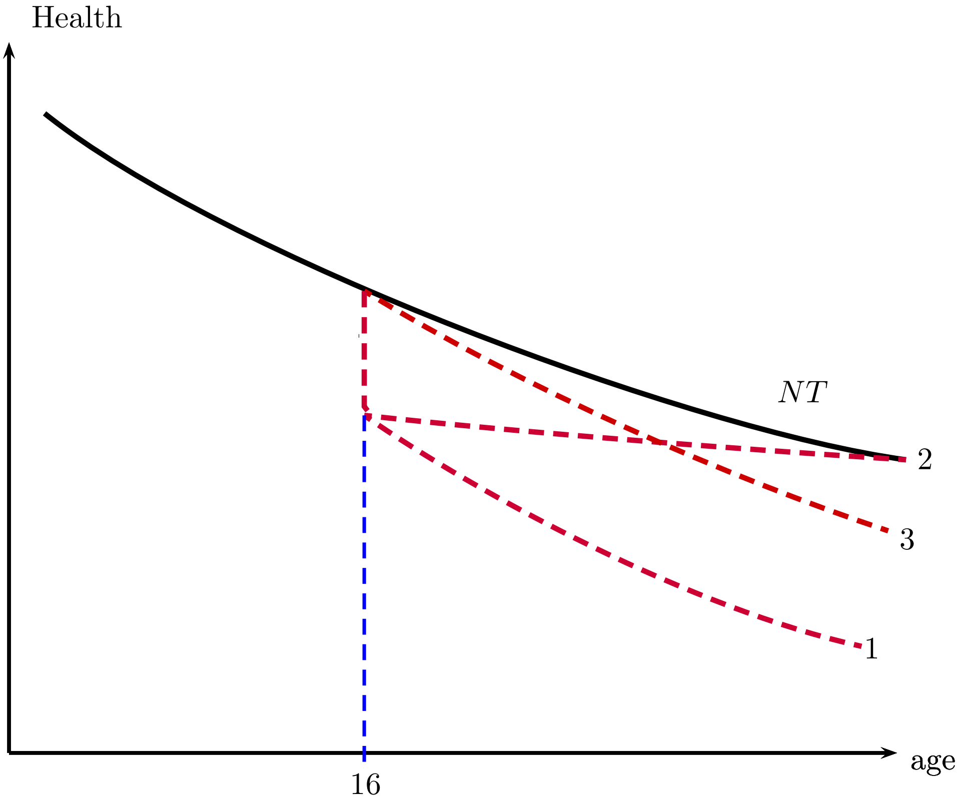
hypotheses. Our data, however, do not permit to disentangle the two effects. A promising avenue for future research would consist in investigating which hypothesis is most likely to hold in the data.

There are some limitations to our study. The most notable is the small sample size, which generates quite imprecise results. In particular, the subsample of men is rather small, so that our results on this population cannot be interpreted as ruling out any damaging impact of poor economic conditions at labour-market entry on health outcomes.

A potential extrapolation of our findings is that the Great recession will have lasting and negative health effects among lower-educated individuals. However, the external validity of our findings depends on the similarity between the 1958 and 1959 GLS cohorts and current cohorts of school-leavers. In the mid-1970s, 50% of pupils left school at compulsory age, while less than 20% do so nowadays. Moreover, there is evidence that the 1973 oil crisis and the current Great recession did not have the same effects on unemployment rates, wages and working conditions in the UK Gregg and Wadsworth (2011). In this context, the extent to which our results can be generalized to young people who entered the labour market during the Great Recession is not clear.

## Tables and Figures

**Figure 1.1** – The evolution of health depending on the scenario.

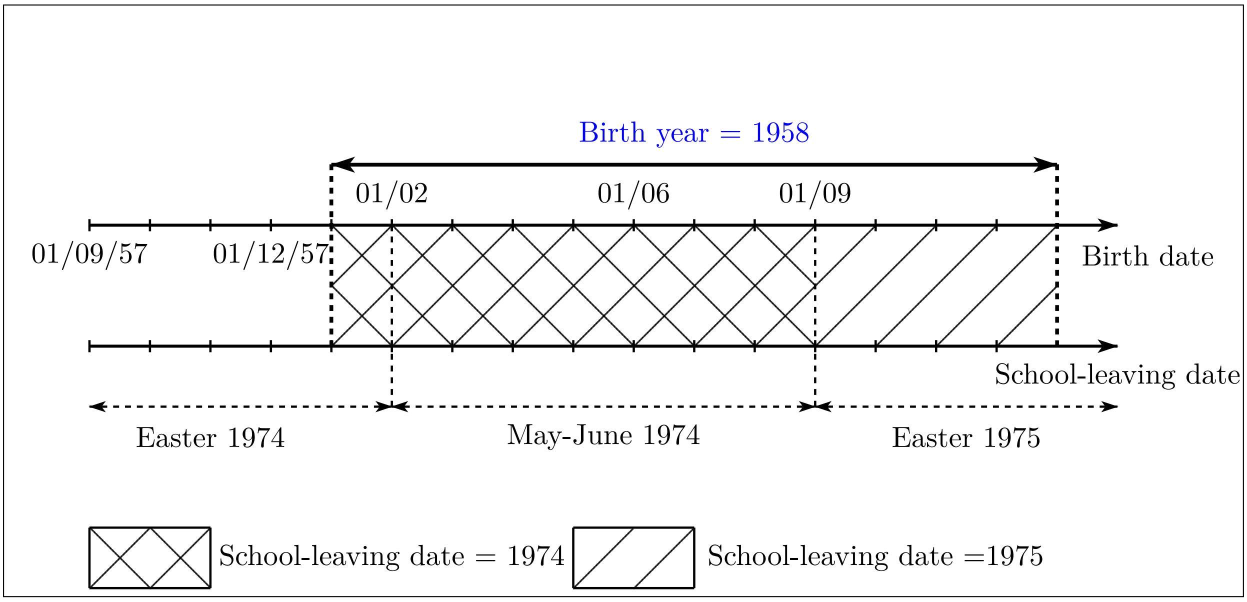



Reading : The red dashed curves show the potential scenarios for treated individuals, whereas the black solid curve presents the evolution of health for those who are untreated. The blue vertical line stands for the entry on the labour market.

**Figure 1.2** – Compulsory schooling rules by month-year of birth

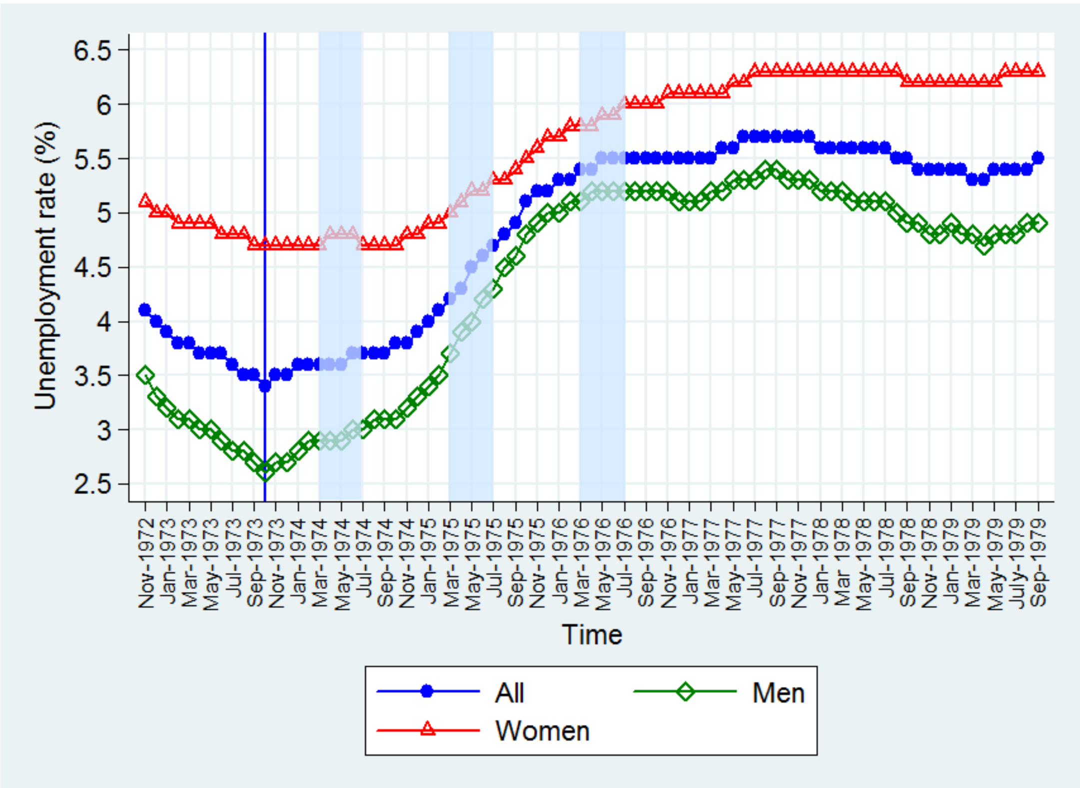
| Birth year (1) | Month of birth (2) | School starting date (3) | Allowed to leave school (4) |
|---|---|---|---|
| 1958 | January | Sept. 1963 | Easter 1974 |
| 1958 | February | Sept. 1963 | May/June 1974 |
| 1958 | March | Sept. 1963 | May/June 1974 |
| 1958 | April | Sept. 1963 | May/June 1974 |
| 1958 | May | Sept. 1963 | May/June 1974 |
| 1958 | June | Sept. 1963 | May/June 1974 |
| 1958 | July | Sept. 1963 | May/June 1974 |
| 1958 | August | Sept. 1963 | May/June 1974 |
| 1958 | September | Sept. 1964 | Easter 1975 |
| 1958 | October | Sept. 1964 | Easter 1975 |
| 1958 | November | Sept. 1964 | Easter 1975 |
| 1958 | December | Sept. 1964 | Easter 1975 |
| 1959 | January | Sept. 1964 | Easter 1975 |
| 1959 | February to August | Sept. 1964 | May/June 1975 |
| 1959 | September to December | Sept. 1965 | Easter 1976 |

**Figure 1.3** – Focus on the 1958 birth cohort



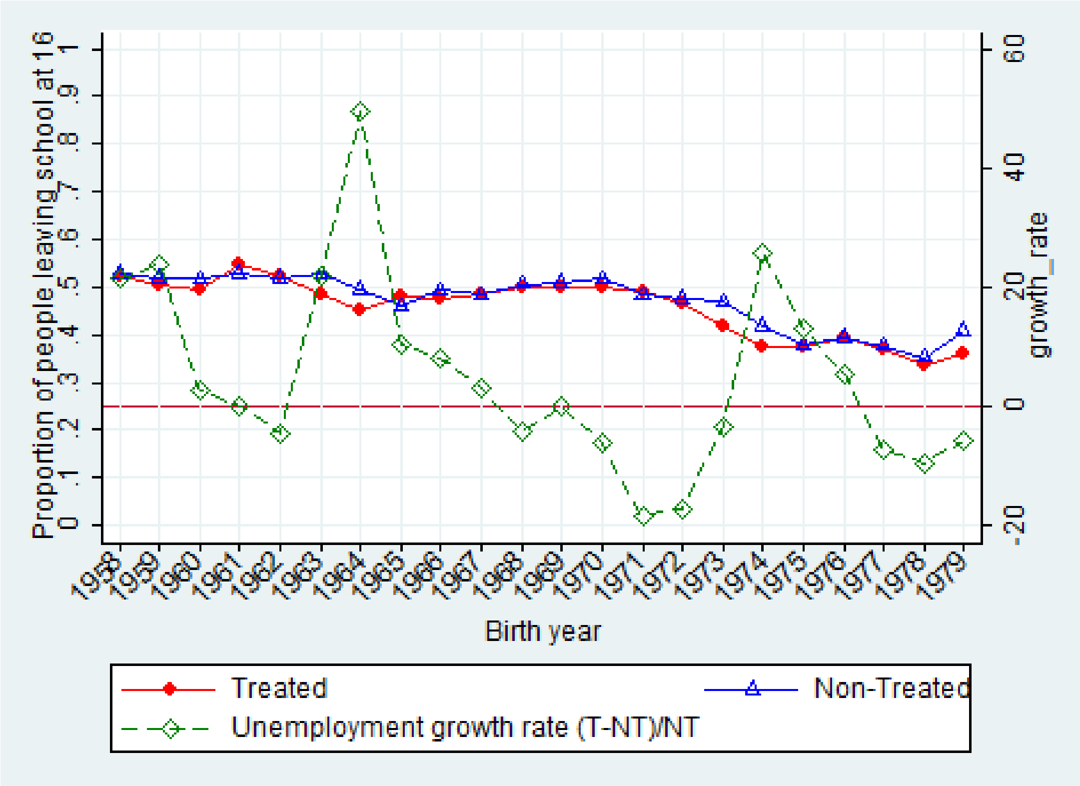

Reading : A pupil born between the 1$^{st}$ of September 1958 and the 31$^{st}$ of December 1958 is allowed to leave school at Easter 1975.

**Figure 1.4** – Unemployment rates for all individuals aged 16-64 over the 1973-1979 period, seasonally adjusted.



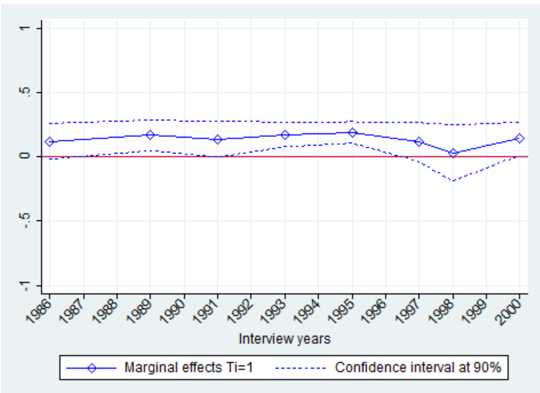

Source : Labour Force Survey (LFS), provided by the Office for National Statistics (ONS).

**Figure 1.5** – Proportion of pupils leaving school at compulsory age among the treated and the non-treated; Growth in school-leaving unemployment rate.




Reading: Figure 1.5 displays the proportion of pupils leaving school at compulsory age among the treated (in red) and non-treated group (in blue); The dashed green line shows the growth in school-leaving unemployment rate (calculated for the March-June period) faced by pupils belonging to the youngest school cohort (treated) – compared to pupils born the same year but belonging to the previous school cohort (non-treated).

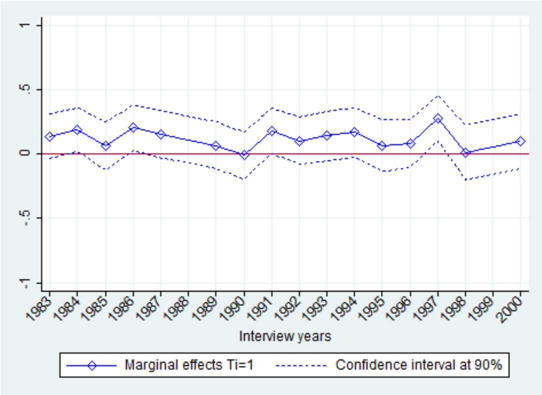
**Figure 1.6** – Health behaviour for men over the lifecourse




(a) Ever smoked

Note : Interview-year specific marginal effects of the treatment are computed by estimating Equation (3.3) and substituting the interaction term $T_i * InterviewYear_i$ for $T_i$.

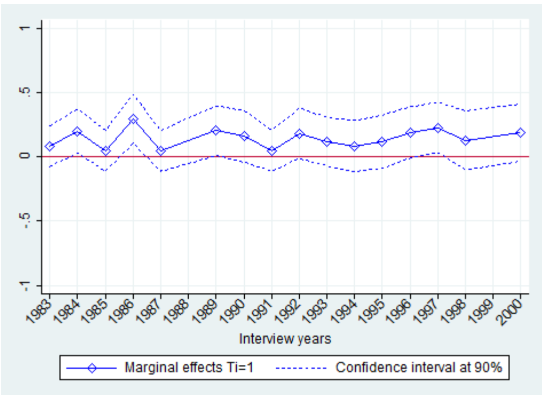
**Figure 1.7** – Health status for women over the lifecourse




(a) Poor health

Note : Interview-year specific marginal effects of the treatment are computed by estimating Equation (3.3) and substituting the interaction term $T_i * InterviewYear_i$ for $T_i$.

**Figure 1.8** – Health care for women over the lifecourse




(a) GP consultation

Note : Interview-year specific marginal effects of the treatment are computed by estimating Equation (3.3) and substituting the interaction term $T_i * InterviewYear_i$ for $T_i$.

**Table 1.1** – Summary statistics of demographic and health variables

| | Men | | | Women | | |
|---|---|---|---|---|---|---|
| | Mean | s.e | *N* | Mean | s.e | *N* |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Demographics** | | | | | | |
| Age | 33.20 | (4.20) | *1096* | $31.19^a$ | (5.00) | *1921* |
| **Health status** | | | | | | |
| Poor self-rated health (yes/no) | 0.30 | (0.46) | *1044* | 0.34 | (0.47) | *1909* |
| Longstanding illness/disability (yes/no) | 0.26 | (0.44) | *1096* | 0.23 | (0.42) | *1917* |
| Restricts activity due to longstanding illness/injury (yes/no) | 0.08 | (0.27) | *1095* | 0.13 | (0.33) | *1920* |
| **Health care** | | | | | | |
| GP consultation last 2 weeks (yes/no) | 0.12 | (0.32) | *1094* | 0.21 | (0.41) | *1920* |
| Outpatient/inpatient spell last 12 months (yes/no) | .16 | (.37) | *1094* | .24 | (.43) | *1918* |
| **Health behaviours** | | | | | | |
| Smoking status | | | *619* | | | *1029* |
| Currently smokes (yes/no) | 0.43 | (0.50) | | 0.42 | (0.49) | |
| Has smoked but does not anymore (yes/no) | 0.33 | (0.47) | | 0.27 | (0.44) | |
| Ever smoked (yes/no) | 0.76 | (0.43) | | 0.69 | (0.46) | |
| Self-reported drinking behaviour | | | *597* | | | *945* |
| High to moderate alcohol consumption (yes/no) | 0.52 | (0.50) | | (0.34) | 0.47 | |

Notes : $^a$ : Women are on average younger than men because they are observed over the whole period (1983-2001) while men are only observed over 1986-2001 (see Table 1.3).

**Table 1.2** – Summary statistics of labour-market characteristics

| | Men | | | Women | | |
|---|---|---|---|---|---|---|
| | Mean | s.e | *N* | Mean | s.e | *N* |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Economic status | | | *1096* | | | *1920* |
| Employed or self-employed (yes/no) | 0.84 | (0.37) | | 0.58 | (0.49) | |
| Unemployed (yes/no) | 0.10 | (0.31) | | 0.06 | (0.23) | |
| Keeping house (yes/no) | 0.01 | (0.09) | | 0.34 | (0.47) | |
| Other (yes/no) | 0.05 | (0.21) | | 0.02 | (0.15) | |
| **For those currently employed or self-employed** | | | | | | |
| Usual gross weekly earnings from main job (in pounds) | 283.72 | (880.68) | *819* | 109.92 | (99.03) | *970* |
| Time with present employer | | | *724* | | | *861* |
| Less than 1 month (yes/no) | 0.02 | (0.13) | | 0.03 | (0.17) | |
| Between 1 and 3 months (yes/no) | 0.04 | (0.20) | | 0.06 | (0.24) | |
| Between 4 and 6 months (yes/no) | 0.04 | (0.20) | | 0.06 | (0.24) | |
| Between 7 and 12 months (yes/no) | 0.08 | (0.27) | | 0.11 | (0.31) | |
| Between 1 and 5 years (yes/no) | 0.20 | (0.40) | | 0.34 | (0.47) | |
| Five years or more (yes/no) | 0.61 | (0.49) | | 0.38 | (0.49) | |

**Table 1.3** – Number of observations by survey wave and birth cohort

| | Men | Women | All |
|---|---|---|---|
| | (1) | (2) | (3) |
| **Survey wave** | | | |
| 1983 | - | 159 | 159 |
| 1984 | - | 153 | 153 |
| 1985 | - | 127 | 127 |
| 1986 | 100 | 130 | 230 |
| 1987 | 92 | 140 | 232 |
| 1988-1989 | 83 | 140 | 223 |
| 1989-1990 | 82 | 102 | 184 |
| 1990-1991 | 74 | 126 | 200 |
| 1991-1992 | 107 | 124 | 231 |
| 1992-1993 | 76 | 97 | 173 |
| 1993-1994 | 85 | 109 | 194 |
| 1994-1995 | 93 | 98 | 191 |
| 1995-1996 | 71 | 121 | 192 |
| 1996-1997 | 92 | 118 | 210 |
| 1998-1999 | 62 | 85 | 147 |
| 2000-2001 | 79 | 92 | 171 |
| **Birth cohort** | | | |
| 1958 | 544 | 972 | 1516 |
| 1959 | 552 | 949 | 1501 |
| Total number of observations | 1096 | 1921 | 3017 |

Notes: (1) The GHS was conducted annually, except for breaks in 1997-1998 when the survey was reviewed, and 1999-2000 when the survey was redeveloped. (2) Month and year of birth in 1983-1985 are only available for women who completed the Family Information section. They are available for all respondents over 1986-2001.

**Table 1.4** – The impact of leaving school in a bad economy on health outcomes (1958-59 cohorts)

| | Men | | | Women | | |
|---|---|---|---|---|---|---|
| | m.e. | s.e. | *N* | m.e. | s.e. | *N* |
| *Probit regressions* | | | | | | |
| **Health status** | | | | | | |
| Poor self-rated health | 0.081 | (0.078) | *1043* | 0.106* | (0.057) | *1907* |
| Longstanding illness/disability | -0.034 | (0.069) | *1095* | 0.051 | (0.051) | *1915* |
| Restricts activity | 0.056 | (0.045) | *1094* | 0.040 | (0.041) | *1918* |
| **Health care** | | | | | | |
| GP consultation last 2 weeks | -0.001 | (0.049) | *1093* | 0.119** | (0.052) | *1918* |
| Hospital consultation | 0.000 | (0.058) | *1095* | 0.026 | (0.051) | *1919* |
| **Health behaviour** | | | | | | |
| Currently smokes | 0.093 | (0.105) | *618* | 0.042 | (0.079) | *1027* |
| Ever smoked | 0.170** | (0.078) | *618* | 0.086 | (0.071) | *1027* |
| Moderate to heavy drinking | -0.028 | (0.107) | *596* | 0.012 | (0.080) | *943* |

Notes : *** p-value<0.01, ** p-value<0.05, * p-value<0.1. Marginal effects (m.e.) are presented (computed as marginal probability effects at the sample mean value of the regressors). Robust standard errors in parentheses (s.e.). Our models include dummy variables for interview and birth year as well as a linear function of age in months – see Equation (3.3).

**Table 1.5** – Placebo test on health outcomes (1953-54 cohorts)

| | Men | | | Women | | |
|---|---|---|---|---|---|---|
| | m.e. | s.e. | *N* | m.e. | s.e. | *N* |
| *Probit regressions* | | | | | | |
| **Health status** | | | | | | |
| Poor self-rated health | -0.059 | (0.095) | *631* | -0.073 | (0.071) | *1204* |
| Longstanding illness/disability | -0.007 | (0.092) | *664* | 0.047 | (0.066) | *1210* |
| Restricts activity | 0.012 | (0.058) | *663* | -0.002 | (0.045) | *1213* |
| **Health care** | | | | | | |
| GP consultation last 2 weeks | -0.047 | (0.051) | *664* | -0.008 | (0.056) | *1211* |
| Hospital consultation | -0.105 | (0.061) | *664* | -0.089 | (0.054) | *1213* |
| **Health behaviour** | | | | | | |
| Currently smokes | -0.035 | (0.127) | *390* | 0.073 | (0.098) | *653* |
| Ever smoked | 0.050 | (0.089) | *362* | 0.052 | (0.081) | *653* |
| Moderate to heavy drinking | -0.177 | (0.132) | *372* | -0.001 | (0.097) | *617* |

Notes : *** p-value<0.01, ** p-value<0.05, * p-value<0.1.. Marginal effects (m.e.) are presented (computed as marginal probability effects at the sample mean value of the regressors). Robust standard errors in parentheses (s.e.). Our models include dummy variables for interview and birth year as well as a linear function of age in months – see Equation (3.3).

**Table 1.6** – Differences-in-differences analysis : the impact of leaving school in a bad economy on health outcomes

| | Men | | | Women | | |
|---|---|---|---|---|---|---|
| | coeff | s.e. | *N* | coeff | s.e. | *N* |
| *Linear probability models* | | | | | | |
| **Health status** | | | | | | |
| Poor self-rated health | 0.014 | (0.049) | *1674* | 0.061* | (0.033) | *3111* |
| Longstanding illness/disability | 0.013 | (0.047) | *1759* | 0.063** | (0.030) | *3125* |
| Restricts activity | 0.017 | (0.030) | *1757* | 0.025 | (0.023) | *3131* |
| **Health care** | | | | | | |
| GP consultation last 2 weeks | 0.008 | (0.033) | *1757* | 0.075*** | (0.028) | *3129* |
| Hospital consultation | -0.052 | (0.039) | *1759* | 0.017 | (0.029) | *3132* |
| **Health behaviour** | | | | | | |
| Currently smokes | 0.010 | (0.068) | *1008* | -0.027 | (0.046) | *1680* |
| Ever smoked | 0.018 | (0.052) | *1008* | 0.013 | (0.042) | *1680* |
| Moderate to heavy drinking | -0.084 | (0.069) | *968* | 0.035 | (0.046) | *1560* |

Notes : *** p-value<0.01, ** p-value<0.05, * p-value<0.1. Marginal effects are obtained by estimating Equation (1.5) and computed as marginal probability effects at the sample mean value of the regressors. Robust standard errors in parentheses (s.e.).

**Table 1.7** – The impact of leaving school in a bad economy on labour-market outcomes (1958-59 cohorts)

| | Men | | | Women | | |
|---|---|---|---|---|---|---|
| | m.e. | s.e. | *N* | m.e. | s.e. | *N* |
| *Probit regressions* | | | | | | |
| Economic status | | | | | | |
| Keeping house | 0.017 | (0.033) | *495* | 0.053 | (0.057) | *1918* |
| Unemployed | 0.017 | (0.050) | *1095* | -0.002 | (0.026) | *1918* |
| For those currently employed | | | | | | |
| Less than 1 month | 0.074** | (0.048) | *512* | 0.034 | (0.037) | *805* |
| Less than 3 months | 0.022 | (0.048) | *613* | 0.053 | (0.059) | *861* |
| Less than 6 months | 0.001 | (0.057) | *723* | 0.029 | (0.068) | *861* |
| Less than 1 year | 0.053 | (0.078) | *723* | -0.035 | (0.077) | *861* |
| Less than 5 years | 0.046 | (0.098) | *723* | -0.091 | (0.089) | *861* |
| More than 5 years | -0.046 | (0.098) | *723* | 0.091 | (0.089) | *861* |
| *Linear regressions* | | | | | | |
| Earnings (log) | -0.041 | (0.094) | *799* | -0.115 | (0.151) | *957* |

Notes : *** p-value<0.01, ** p-value<0.05, * p-value<0.1. Marginal effects (m.e.) are presented (computed as marginal probability effects at the sample mean value of the regressors). Robust standard errors in parentheses (s.e.). Our models include dummy variables for interview and birth year as well as a linear function of age in months – see section 1.4.

# Appendix

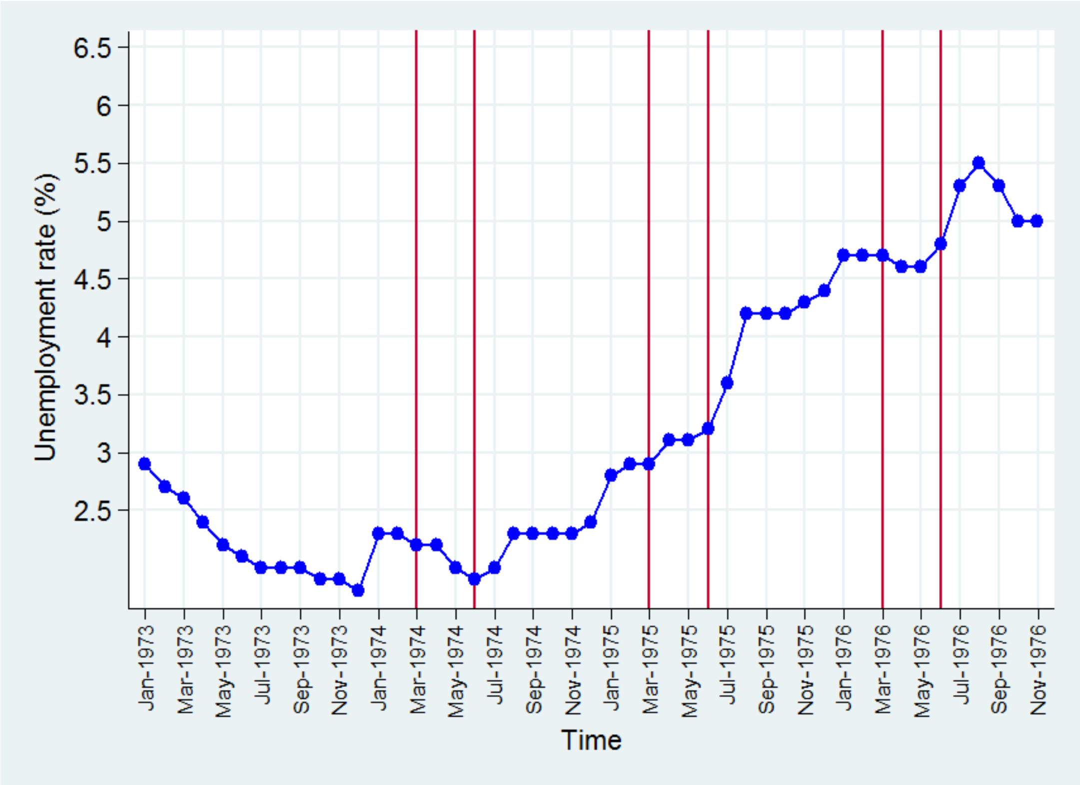
## A-1.1. Tables and Figures

**Table A-1.1** – The impact of leaving school in a bad economys on health outcomes : An alternative approach using school-leaving unemployment rates (LFS)

| | Men | | | Women | | |
|---|---|---|---|---|---|---|
| | m.e. | s.e. | *N* | m.e. | s.e. | *N* |
| *Probit regressions* | | | | | | |
| **Health status** | | | | | | |
| Poor self-rated health | 0.127$^{\mu}$ | (0.082) | *1043* | 0.101* | (0.059) | *1907* |
| Longstanding illness/disability | -0.031 | (0.076) | *1095* | 0.066 | (0.053) | *1915* |
| Restricts activity | 0.074* | (0.043) | *1094* | 0.034 | (0.041) | *1918* |
| **Health care** | | | | | | |
| GP consultation last 2 weeks | 0.026 | (0.054) | *1093* | 0.078$^{\mu}$ | (0.051) | *1918* |
| Hospital consultation | -0.029 | (0.063) | *1095* | 0.018 | (0.054) | *1919* |
| **Health behaviour** | | | | | | |
| Currently smokes | 0.076 | (0.114) | *618* | 0.043 | (0.084) | *1027* |
| Ever smoked | 0.144$^{\mu}$ | (0.097) | *618* | 0.126 | (0.078) | *1027* |
| Moderate to heavy drinking | 0.022 | (0.117) | *596* | 0.023 | (0.085) | *943* |

Notes : *** p-value<0.01, ** p-value<0.05, * p-value<0.1, $^{\mu}$ p-value<0.15. Marginal effects (m.e.) are presented (computed as marginal probability effects at the sample mean value of the regressors). Robust standard errors in parentheses (s.e.). Our models include dummy variables for interview and birth year as well as a linear function of age in months – see Equation (1.6).

**Figure A-1.1** – Seasonal fluctuations of the labour-market (1973-1976). Monthly unemployment rates.




Source : Monthly registrant count (borrowed from Denman and McDonald (1996))

## A-1.2. Data Appendix : Sample and variable construction

### Changes to sampling procedures and sample sizes over time

According to the GHS Time Series Dataset User Guide (2007), "the sampling procedure used on the GHS has changed over time, resulting in different sample sizes between survey years. However, the changes to the GHS sample procedures and sample size were relatively small. As a result it was decided by ONS that these changes were likely to have little impact on the reliability of the estimates. Particularly as a representative sample of the population has been achieved for each survey year."

Non-response weights are only available in the GHS after 2000. As a consequence, all our estimates are unweighted.

### Inconsistencies in variables over time

According to the GHS Time Series Dataset User Guide (2007), "in general variables in the GHS have remained fairly consistent over time. However as the GHS has been revised and research interests have changed, some variables have been modified over the past 30 years to reflect this. For example the marital status variable was revised in the 1986 survey to include a category for cohabitation. Similarly, some questions were only included on a few survey years, or in more recent rounds of the survey series, which limits analysis over time."

Those variables that were only available for a few years, or had substantially changed over time were not used in the analysis.

### A-1.3. School leaving age legislation in England and Wales

Relevant extracts of the 1962 Education Act are borrowed from Del Bono and Galinda-Rueda (2007).

***Education Act 1962: relevant extracts from Section 9***

*Applies to 15 year old individuals in 1963, i.e. people born in 1947 or afterwards.*

*(2) If he attains that age on any date from the beginning of September to the end of January, he shall be deemed not to have attained that age until the end of the appropriate spring term at this school.*

*(3) If he attains that age on any date on or after the beginning of February but before the end of the appropriate summer term at his school, he shall be deemed not to have attained that age until the end of that summer term.*

*(4) If he attains that age on any date between the end of the appropriate summer term at this school and the beginning of September next following the end of that summer term (whether another term has then begun or not) he shall be deemed to have attained that age at the end of that summer term. [...]*

*(7) In this section, "the appropriate spring term", in relation to a person, means the last term at this school which ends before the month of May next following the date on which he attains the age in question, and "the appropriate summer term" [...] means the last term at this school which ends before the month of September next following that date [...].*

***Education School leaving Act 1976: relevant extracts from Section 1***

*Subsections (3) and (4) in Section 9 of the Education Act of 1962 were substituted by the following subsections of Section 1 of the Education School leaving Act 1976 in order to give a*

*more precise meaning to the notion of school leaving dates, particularly for those born after the end of January.*
*(3) If he attains that age after the end of January but before the next May school leaving date, he shall be deemed not to have attained that age until that date.*
*(4) If he attains that age after the May school leaving date and before the beginning of September next following that date, he shall be deemed to have attained that age on that date. A new subsection was added at the end of Section 9 of the Education Act of 1962, according to which:*
*(8) In this section the May school leaving date means the Friday before the last Monday in May.*

# Chapter 2

# Gaining weight through retirement? Results from the SHARE survey.


## Abstract

This chapter estimates the causal impact of retirement among the 50-69 year-old on Body Mass Index (BMI), the probability of being either overweight or obese and the probability of being obese. Based on the 2004, 2006 and 2010-11 waves of the Survey of Health, Ageing and Retirement in Europe (SHARE), our identification strategy exploits the European variation in Early Retirement Ages (ERAs) and the stepwise increase in ERAs in Austria and Italy between 2004 and 2011 to produce an exogeneous shock in retirement behaviour. Our results show that retirement induced by discontinuous incentives in early retirement schemes causes a 13 percentage point increase in the probability of being obese among men within a two to four-year period. We find that the impact of retirement is highly non-linear and mostly affects the right-hand side of the BMI distribution. Additional results show that our results are driven by men having retired from strenuous jobs and who were already at risk of obesity. No effects are found among women.

## 2.1. Introduction

> "Like a lot of athletes, I've gained weight since I've retired. [...] The doctor said, 'Hey dude, if you don't lose some weight you're either going to get diabetes, have a stroke or drop dead. It's either A, B or C.'" Charles Barkley. Mr. Barkley is a former NBA champion and has recently retired. He acts as a spokesman for Weight Watchers "Lose Like a Man" campaign.

In its 1998 report, the World Health Organization (WHO) ranked the obesity epidemic among the leading ten global public health issues. Obesity rates in the world have more than doubled over the last 30 years (WHO (2012)). In the 27 European Union member states, approximately 60% of the adult population – 260 millions of adults – is either overweight (Body Mass Index (BMI) from 25 to 29.9 kg/m$^2$) or obese (BMI 30 kg/m$^2$ and above) (International Obesity Task Force (IASO/IOTF (2010)). Obesity has become a pan-European epidemic (IASO/IOTF (2002)) and prevalence rates in the EU-27 range from 7.9% in Romania to 24.5% in the United-Kingdom (OECD (2010a)).

Obesity is a risk factor for numerous highly-prevalent and costly chronic diseases (cardiovascular diseases, type-2 diabetes, hypertension and certain types of cancer) and for disability. It reduces the quality of life, shortens life expectancy and lowers the levels of labour productivity (Must et al. (1999); Rosin (2008)). Moreover, it places a heavy financial burden on the individual and on society – particularly on public transfer programmes and private health plans (Finkelstein et al. (2003)). At the individual level, Emery et al. (2007) find that healthcare costs for French obese individuals are on average twice the costs for normal-weight individuals. At the aggregate level, obesity-related healthcare expenditures account for 1.5 to 4.6% of total health expenditures in some European countries (see Schmid et al. (2005) and Emery et al. (2007) for evidence on France and Switzerland respectively).

In most European countries, obesity rates reach their peak around age 60.5[1] (Sanz-de Galdeano (2005)). Recent studies have highlighted the particularly strong impact of overweight, obesity and increased BMI on morbidity and disability among adults aged 50 and older (Jenkins (2004); Andreyeva et al. (2007); Peytremann-Bridevaux and Santos-Eggimann (2008)), thereby

[1] This figure does not allow us to disentangle age and cohort effects. Using the 2004, 2006 and 2010 waves of the Survey of Health, Ageing and Retirement in Europe (SHARE), we find that obesity rates among the 50-70-year-old reach their peak between age 55 and 65 for all cohorts born between 1940 and 1954.

attracting policymakers' attention to the substantial burden that obesity places on the general health and autonomy of adults aged over 50.

Understanding the causes of obesity among the elderly is therefore a key issue. Unlike other age groups – such as children or adolescents – it hasn't received much attention yet. As the elderly are characterised by low labour participation and high job-exit rates, one might wonder whether transitions out of employment have an impact on the weight trajectories of individuals aged 50 years and older. In this paper, we focus on the most common transition out of employment, i.e, retirement.

There are some reasons to believe that retirement might trigger weight changes. The Grossman model of the demand for health (Grossman (1972)) is consistent with the interpretation that individuals are likely to adopt health-producing activities after retirement. Although retirees have a tighter budget constraint, they have more time to allocate to leisure : they may engage in physical activity or healthier diets for instance, which are time-consuming but not money-consuming. Empirical findings seem to corroborate this view. In a three-year follow-up of French middle-aged adults, Touvier et al. (2010) find that retirement is associated with an increase in leisure-time physical activities of moderate intensity, such as walking. As for food intake, finding are more mixed. On US longitudinal data, Chung et al. (2007) find that households spend less on eating out ($10 per month on average) following retirement, while their monthly spending on food at home does not change. In a recent review of the literature, however, Hurst (2008) argues that due to an increase in food home production, the overall food intake does not decline following retirement. Overall, these results suggest that retirement would rather operate on weight through changes in physical activity than via food consumption.
At the same time, new retirees may lose some incentive to invest in health as their income (pension benefits) is no longer dependent on health. This could lead to lower health investments, and to a lower health stock in the long-run. Besides, retirement might also increase the risk of social isolation and depression (Friedmann and Havighurst (1954); Bradford (1979)), leading individuals to potentially reduce their efforts in health-producing activities and develop addictive behaviours (alcohol or tobacco consumption). Finally, the loss of a structured use of time may also encourage snacking in-between meal times and sedentary habits (television watching). In the study mentioned before, Touvier et al. (2010) find that retirement is also associated with an increase in time spent watching TV.

Overall, the direction of the effect is not clear. This effect, however, is likely to be highly heterogeneous, in particular across job types. As retirement induces a direct reduction in job-related exercise, individuals having retired from strenuous jobs are at a higher risk to gain weight if they do not compensate by increasing their leisure-time physical activity or by decreasing their food intake. Conversely, retirees from sedentary jobs may lose weight if their leisure-time activities after retirement are more physically demanding than time at work.

The purpose of the present paper is to estimate the causal impact of retirement on BMI, the probability of being either overweight or obese and the probability of being obese. Identifying such a causal impact is problematic in the presence of confounding factors and reverse causality. Retirement is indeed often a choice, and often based on unobservable characteristics which may be correlated with weight (time preference[2], health or psychological deteriorations). Reverse causality may also be a concern. As overweight and obese individuals are on average paid less, less promoted (Cawley (2004); Morris (2006); Brunello and d'Hombres (2007); Schulte et al. (2007)) and in worse health, their incentives to retire might be higher than normal-weight individuals. Burkhauser and Cawley (2006) show that fatness and obesity are indeed strong predictors of early receipt of old-age benefits in the USA.

To tackle this endogeneity issue, we use an instrumental variable approach. Our identification strategy exploits the fact that as individuals reach the Earliest Retirement Age (ERA) at which they are entitled to either reduced pensions or full pensions – conditional on a sufficient number of years of social security contributions – the probability that they retire strongly increases. Said differently, this discontinuous incentive in the social security system provides a strong exogeneous shock on retirement behaviour. We exploit the variation in ERAs across European countries as well as its variation over time (in countries that implemented a stepwise increase in the ERA during the period under study) to solve the major identification problems related to confounding factors and reverse causality. We implement a fixed-effect instrumental variable model in order to control for both time-invariant factors (such as genetics) and time-varying ommited variables and/or reverse causality. We finally estimate the short-term causal effect of a transition to retirement on weight. We use the 2004, 2006 and 2010 waves of the Survey of Health, Ageing and Retirement in Europe (SHARE). Our results show that

[2]See Smith et al. (2005), Anderson and Mellor (2008) and Ikeda et al. (2010) for empirical evidence of the positive relationship between time preference and BMI.

retirement causes a 13 percentage point increase in the probability of being obese within a two to four-year period[3] among men. We find that the impact of retirement is highly non-linear and mostly affects the right-hand side of the BMI distribution. Additional results show that this effect is driven by men having retired from strenuous jobs and who were already at risk of obesity. No effects are found among women.

This paper relates to several strands of literature. First and foremost, it contributes to the literature on the effects of retirement on weight. Most papers in this literature estimate mere correlations, disregarding the possibility that retirement be endogenous. Results have been quite consistent so far. Nooyens et al. (2005) find that the effect of retirement on changes in weight and waist circumference depends on one's former occupation : weight gain is higher among men who retired from an active job. Forman-Hoffman et al. (2008) find no significant relation for men, but a weight gain for women retiring from blue-collar jobs. Gueorguieva et al. (2010) find a significant increase in the slopes of BMI trajectories only for individuals retiring from blue-collar occupations. To the best of our knowledge, Chung et al. (2009) and Goldman et al. (2008) are the only studies tackling the endogeneity issue. Both use longitudinal data from the Health and Retirement Study – the US equivalent of the European SHARE survey – and estimate fixed-effect models with instrumental variables. They use social security and Medicare eligibility (ages 62 and 65 respectively) as instruments for retirement.[4] Chung et al. (2009) conclude that people already overweight and people with lower wealth retiring from physically-demanding occupations suffer from a modest weight gain. Goldman et al. (2008) find that males retiring from strenuous jobs gain weight (by 0.5 units of BMI) during the first six years of retirement, while those retiring from sedentary jobs lose some. We improve with respect to this literature in three respects : first, we identify a causal effect of retirement on weight, while most papers document a mere correlation. Second, the variation in ERAs across Europe and over time allows us to explore the effect of retirement on weight at different ages, not just ages 62 and 65, as in Chung et al. (2009) and Goldman et al. (2008). Weaker assumptions in terms of weight trajectories by cohort and age are needed in our empirical setup. Finally, our paper is the first one to exploit European data. Most of the above-mentioned studies – except Nooyens et al. (2005) – use US data from the Health and Retirement Survey (HRS). Given the

[3]There is a two-year period between the 2004 and 2006 waves of SHARE and a four-year period between waves 2006 and 2010.

[4]Chung et al. (2009) also use spouse pension eligibility as an additional instrument. However, recent work highlights asymmetries in spouses' retirement strategies (Gustman and Steinmeier (2009); Stancanelli (2012)). Using spouse pension eligibility as an additional instrument might thus be a questionable strategy.

differences in terms of labour markets, social security schemes and social policies, it is not clear whether the results obtained for the USA should hold for Europe.

This paper also relates to a substantial recent literature that explores the effects of retirement on health and related health outcomes – mental health, cognitive functioning and well-being. The results in this literature are very ambiguous, and whether or not retirement has a detrimental effect on health is still an open debate (Charles (2004); Neuman (2008); Coe and Lindeboom (2008); Coe and Zamarro (2011)[5]; Rohwedder et al. (2010); Behncke (2011); Bonsang et al. (2012); Blake and Garrouste (2012); Eibich (2014)). These conflicting results are mainly due to the fact that analysing the long-term health effect of retirement – which is not easily disentangled from the effect of age – remains a hard task. A promising way to solve this "retirement puzzle" is to look, as we do, at behavioural outcomes following retirement. These behavioural outcomes can be rapidly modified in the short-run and precede the longer-run health outcomes (such as chronic diseases, mortality etc.). We thus analyse how weight change is modified in the short-run. We also investigate to which extent this effect is heterogeneous across several dimensions, such as gender, occupational strenuousness, baseline weight category etc. As weight change is likely to be an important mechanism by which retirement affects health, this chapter contributes to this recent and growing literature by exploring one of the potential mediating channels between retirement and health.

Finally, this chapter contributes to a growing body of literature that investigates the impact of various dimensions of professional activity on body weight and obesity, such as papers focusing on unemployment (Marcus (2012)), working conditions (Lallukka et al. (2008b)), occupational mobility (Ribet et al. (2003)), job insecurity (Muenster et al. (2011)), physical strenuousness at work (Böckerman et al. (2008)), working overtime (Lallukka et al. (2008a)), and income (Cawley et al. (2010), Schmeiser (2009), Colchero et al. (2008)).

This chapter develops as follows. Section 2 presents our empirical approach and Section 3 describes the data (the 2004, 2006 and 2010 waves of SHARE). Section 4 presents the results and displays several robustness checks and Section 5 concludes.

[5] Our identification strategy is similar in spirit to Coe and Zamarro (2011), who use the 2004 wave of SHARE and use country-specific early and full retirement ages as instruments for retirement behaviour. However, we improve with respect to this paper in two respects. First, we take advantage of the panel structure of the SHARE data, which allows us to control for individual time-invariant unobservable characteristics. Second, we exploit reforms in early retirement ages in Austria and Italy over the 2004-2011 period to produce an exogeneous shock in retirement. Finally, rather than investigating the effect of retirement on health, we investigate the effect of retirement on an under-investigated dimension of health and a major risk factor for numerous diseases among the elderly, i.e, weight change and obesity.

## 2.2. Empirical approach

We investigate the impact of retirement on BMI, the probability of being either overweight or obese and the probability of being obese. As a first step, we pool the observations from the 2004, 2006 and 2010 waves of the SHARE survey and estimate the following equation by a standard Pooled Ordinary Least Squares (POLS) model :

$$Y_{it} = \alpha + \gamma R_{it} + X_{it}\beta + D_i + D_t + u_{it} \tag{2.1}$$

where $Y_{it}$ denotes the weight outcome of individual $i$ at time $t$.[6] $R_{it}$ is a binary variable indicating whether individual $i$ is retired at time $t$, $X_{it}$ a vector of individual characteristics either time-varying or time-invariant, $D_i$ a country dummy, $D_t$ a time dummy and $u_{it}$ the error term. However, the retirement status $R_{it}$ can potentially be correlated with the error term $u_{it}$, in which case the POLS estimate of $\gamma$ is inconsistent. Endogeneity may arise from several sources. Omitted variables, such as unobservable time preference or health deteriorations may have an impact both on the probability of retiring and on weight changes. Similarly, reverse causality may also be a concern : obese individuals are more likely to seek early retirement benefits (Burkhauser and Cawley (2006)).

Faced with these endogoneity problems, we consider a Fixed-Effects (FE) model such as :

$$Y_{it} = \alpha + \gamma R_{it} + K_{it}\beta + D_t + \alpha_i + v_{it} \tag{2.2}$$

where $Y_{it}$ still denotes the weight outcome, $R_{it}$ the individual retirement status, $K_{it}$ a vector of time-varying individual characteristics, $D_t$ a time dummy, $\alpha_i$ an individual fixed-effect – including the country fixed-effect – and $v_{it}$ the error term.

The FE model allows regressors to be endogeneous, provided that they are correlated only with $\alpha_i$, the time-invariant component of the error, but not with the idiosyncratic error $v_{it}$. If some unobservable time-varying characteristics are correlated with $R_{it}$, however, $\hat{\gamma}$ continues to be biased. Moreover, reverse causality is still a concern.

In order to tackle the endogeneity problem, we estimate a Fixed-Effect Instrumental Variable

[6] $Y_{it}$ can be either continuous (the BMI) or binary (being either overweight or obese/being neither overweight nor obese; being obese/not being obese). POLS (presented) and pooled probit models (not presented but available upon request) yield very similar results when the dependent variable is binary.

(FEIV) model. This model allows us to control for both time-invariant factors (such as genetics, food preferences over the life-course or time preference) and time-varying ommited variables (such as health deteriorations) as well as reverse causality. Our identification strategy exploits the fact that as individuals reach the Earliest Retirement Age (ERA) in their countries, the probability that they retire strongly increases.[7] This exogeneous shock in retirement behaviour allows us to estimate the causal impact of a transition to retirement on weight in the short-run – within a two to four-year period.[8]

Retirement decisions in industrialised countries depend on a number of institutional features. In particular, the earliest age at which individuals are entitled to pension benefits has been shown to exert a powerful influence on their retirement behaviours (Gruber and Wise (1999)). This ERA is defined as the earliest age at which individuals are entitled to either reduced pensions or full pensions – conditional on a sufficient number of years of social security contributions. The Official Retirement Age (ORA) is the age at which workers are entitled to either minimum-guaranteed pensions or full old-age pensions irrespective of their contributions or work histories. It appears to be typically less important in predicting retirement behaviour than the ERA (Gruber and Wise (1999)). Few individuals actually work until the official retirement age. As a consequence, there is a gap between the official retirement age and the average effective age at which older workers withdraw from the labour force in almost all industrialised countries.

Earliest, official and effective retirement ages in Europe are presented in Table 2.1. As evidenced in columns 1 and 2, the official retirement age varies very little across countries and genders. In contrast, the ERA varies quite a lot across countries and genders (columns 3 and 4). Effective retirement ages are lower than official retirement ages in almost every country (see columns 5 and 6 for men and women respectively). A number of countries in our sample implemented substantial reforms in ERAs over the period under study. In Austria for instance, the 2004 pension reform introduced a gradual increase in the ERAs for men and women. Immediately before the reform, workers in Austria could still retire at ages 61.5 (men) and 56.5

[7] One could use a health shock between two subsequent waves of the survey as an alternative instrument for retirement behaviour. However in practice, the exclusion restriction – according to which this health shock does not affect weight except through the increased probability of retiring – is not likely to hold in the data.

[8] There is a two-year period between the 2004 and 2006 waves of SHARE and a four-year period between waves 2006 and 2010. In this setup, we assume that the effect of retirement on weight in a two-year period is the same as in a four-year period. Another option is to consider the two and four-year periods separately and run the regressions on two different samples. However, we chose to consider the single sample because (i) we thus deal with a larger sample, which is important when investigating the heterogeneous impact of retirement and focusing on subsamples (ii) we have three observations per individual over the period.

(women). After the reform, the ERAs were increased by two months for each quarter of birth for men born in the first two quarters of 1943 and women born in the first two quarters of 1948. Following these increases, the ERAs were increased by one month for each quarter of birth for men born in the third quarter of 1943 and later and for women born in the third quarter of 1948 and later. Furthermore, the 2004 pension reform also created special corridor pensions for men born in the last quarter of 1943 and later, thereby making the ERA beyond age 62 non-binding in many cases (Manoli and Weber (2012)). Italy also introduced a stepwise increase in the minimum age to request early retirement, from age 57 in 2004 to age 60 in 2011. More information about the Austrian and Italian reforms are available in Table 2.1.

We take advantage of the ERA variation across countries and over time to explore the causal effect of retirement on weight. We instrument the retirement status $R_{it}$ by a dummy variable indicating whether individual $i$'s age at time $t$ is above or below the ERA in force at time $t$ in his country $c$.[9] [10] Let $age_{it}$ be individual $i$'s age at time $t$ and $ERA_{ct}$ the ERA in $i$'s country $c$ at time $t$. Our instrument is defined as :

$$Z_{ict} = \mathbb{1}_{\{age_{it} > ERA_{ct}\}} \tag{2.3}$$

A good instrument should be strongly correlated with actual retirement behaviour but should not directly affect weight outcomes.

As shown in Table 2.1, $Z$ appears to be well correlated with retirement status. Suggestive evidence is provided by columns (7) and (8) : in each country, there is a large gap in the fraction of individuals retired before and after the ERA cutoff. For example, only 17% of individuals in the pooled sample in France are retired before age 60 – when they are first entitled to social security benefits – but this proportion increases to 88% after age 60. Taking advantage of the panel structure of our data, we then compute for each country the proportion of individuals retiring when reaching their country's ERA between two subsequent waves of the survey (see column (9)). This proportion is high in most countries. For instance in Belgium, 34.3% of the individuals reaching age 60 between two waves of the survey actually retire between these two waves.

[9] For countries where there was no increase in ERAs between 2004 and 2011 – i.e. all countries, except Italy and Austria – our identification uses a non-linear version of age, therefore identifying using the functional form of age.

[10] Note that our results do not change when using both the ORA and ERA as joint instruments for retirement behaviour.

At the same time, once controlling for age, reaching the ERA cutoff is highly unlikely to be correlated with weight outcomes except through the increased probability of retiring. This exclusion restriction holds if we assume there is no discontinuity in the weight trajectories of cohorts at ERAs except for the effect of retirement at these given ages. As we consider different cohorts and since the ERA is both country and time-varying, this assumption is likely to hold in our data. We show in the robustness section that it is the case.

Equation (2) is then estimated by fixed-effect two-stage least squares where $Z_{ict}$ is an instrument for $R_{it}$. In the first stage, the retirement status $R_{it}$ is regressed on $Z_{ict}$ and other covariates. In the second stage, equation (2) is estimated by a FE regression/FE linear probability model where $R_{it}$ is replaced with its predicted value from the first stage. The covariance matrix of $\hat{\gamma}$ is corrected accordingly.

Our FEIV estimate $\hat{\gamma}$ can be given a causal interpretation as a Local Average Treatment Effet (LATE) without requiring constant treatment assumption. In our case, the "treatment" is defined as retiring between two subsequent waves of the survey. More specifically, $\hat{\gamma}$ is identified on the subset of individuals whose behaviour is shifted by our instrument, i.e, the compliers. In this setup, compliers are (i) individuals who became eligible to early retirement schemes between two subsequent waves of the survey and *did* retire then – but who would not have retired if they had not become eligible (ii) individuals whose eligibility to early retirement schemes did not change between two subsequent waves of the survey and *did not* retire then – but who would have retired if they had become eligible. As the ERA is probably more binding for individuals with long careers, we expect compliers to be less educated people.

Overall, our estimation strategy allows to us to measure the causal effect of a transition to retirement on weight within a two to four-year period among this subpopulation of compliers.

Our empirical setup allows us to explore the effect of retirement on a wide range of ages, not just ages 62 and 65 as in the US studies. Moreover, weaker assumptions in terms of weight trajectories by cohort and age are needed in this setup.

Finally, as Coe and Zamarro (2011) underline, there do exist other ways to exit the labour force, e.g., through unemployment or disability programmes. However, to the extent that these patterns are stable within countries over the period under study, the individual fixed-effect will pick up this variation and it will not bias our results.

## 2.3. Data

### 2.1. Presentation of the sample

We use data from the Survey of Health, Ageing and Retirement in Europe (SHARE). SHARE is a multidisciplinary and cross-national panel database containing individual information on health, socio-economic status and social and family networks. Approximately 85,000 individuals over 50 years old and their spouses/partners (independent of their age) from 19 European countries (including Israel) have been interviewed so far. By now, four waves have been conducted and further waves are being planned to take place on a biennial basis. We use the 2004, 2006 and 2010 waves of SHARE.[11] In order to have a balanced panel, our sample includes the ten European countries that took part in the 2004 SHARE baseline survey and further participated in waves 2006 and 2010, i.e., Austria, Germany, Sweden, The Netherlands, Spain, Italy, France, Denmark, Switzerland and Belgium.

Our sample contains all individuals interviewed in waves 2004, 2006 and 2010[12], aged 50 to 69 years old[13], who declared in each wave being either employed or retired. In other words, we only consider the traditional and most frequent pattern of retirement, where individuals transit directly from work to retirement. Transitions from employment to unemployment, invalidity or inactivity are thus excluded. We also exclude transitions from retirement to employment, unemployment, invalidity or inactivity. In the empirical analysis we thus compare individuals whose job status remains stable across waves (either retired or employed) and individuals who retire across waves. As there is no early retirement option in Denmark and since early retirement was abolished in 2005 in the Netherlands, both countries are excluded from the analysis. Finally, we exclude individuals reporting a height below 1.20 meters as well as individuals reporting a weight either below 30 kilograms or above 200 kilograms. Overall, our dataset contains 2703

[11] The 2008-2009 wave of SHARE, SHARELIFE, is a retrospective survey that focuses on people's life histories. Although it can be linked to the existing data of SHARE, it is not of direct use here and we do not use it.

[12] We thus consider a balanced panel. Attrition rates are rather high in SHARE – 30% between the 2004 and 2006 waves of the survey. In our setup, high attrition rates are a concern if non-response is systematically related to weight. We show that this is not the case in the robustness section. Additional robustness checks show that our results do not significantly vary when re-running our regressions on an unbalanced panel.

[13] The 50-69 age window broadly corresponds to the ages at which individuals reach the ERA in their country and become entitled to pension benefits.

individuals[14] from eight countries (Austria, Germany, Sweden, Spain, Italy, France, Switzerland and Belgium) across the three waves.

## 2.2. Variables

We use a question on self-declared current job situation to determine whether an individual is retired or not. According to this definition, anyone who declares herself as retired, whether she has been or not in a paid job during the month preceding the interview – even for a few hours – is considered as retired. Conversely, anyone who declares herself to be employed or self-employed is considered as currently working. The self-declared retirement status seems to be a reliable information in SHARE : it is strongly associated with the eligibility for either public or private pensions in the dataset.[15] We also use an alternative and more restrictive definition of retirement as a robustness check. According to this definition, an individual is considered as retired if (i) his self-declared job situation is "retired" and (ii) he did not do any paid work during the preceding month. Conversely, an individual is considered as employed if his self-declared job situation is "employed or self-employed".[16]
Tables 2.2 and 2.3 provide summary statistics for the full sample – pooled over 2004-2010 – for men and women respectively. Each table also presents characteristics for the individuals either continuously employed across waves (column 2), continuously retired across waves (column 3), or having retired across waves (column 4). According to Tables 2.2 and 2.3, 45% of men and 43% of women in the full sample were employed or self-employed, the rest being retired. Eight hundred and sixteen individuals (23% of the individuals working in 2004) retired between 2004 and 2010. According to our alternative definition of retirement, only 395 individuals (13% of the individuals working in 2004) retired between 2004 and 2010.

The BMI is calculated in each wave as the self-declared weight in kilograms divided by the square of the self-declared height in meters (kg/m$^2$). We derive clinical weight categories from the BMI : underweight (BMI under 18.5 kg/m$^2$), normal (BMI from 18.5 to 24.9 kg/m$^2$), overweight (BMI from 25 to 29.9 kg/m$^2$) and obese (BMI 30 kg/m$^2$ and above). We also compute

[14] Once conditioning on having no missing value on weight, height and any covariate included in the model, our sample goes down to 2493 individuals across the three waves (1353 men and 1140 women), i.e., 7479 observations in the pooled sample (4059 men and 3420 women).

[15] Among the 3281 individuals retired in the pooled sample, 84% declared that they had received an income from either a public or occupational old age pension during the year preceding the interview.

[16] SHARE also includes information about the year and the month of retirement. However, this measure is not reliable in our data and we do not use it. Hence, we know if a given individual retires between two waves of the survey, but we do not have any information on the exact month and year of retirement.

individual weight change (in kg) as well as a dummy variable indicating if the individual experienced a weight change of at least 10% between two subsequent waves of the survey. The BMI is a rather crude measure of body composition, as it does not distinguish fat from lean mass (Prentice and Jebb (2001); Burkhauser and Cawley (2008)). However, it has been shown to be highly correlated with more precise measures of adiposity. When reported – as it is the case here –, the BMI may additionally suffer from measurement error (Niedhammer et al. (2000); Burkhauser and Cawley (2008)). Following Brunello et al. (2013), we note that the rank correlation between country level self-reported and objective measures of weight is however very high in Europe (Sanz de Galdeano (2007)).

The average BMI of the full sample was 26.95 kg/m$^2$ for men and 25.79 kg/m$^2$ for women, slightly above the overweight threshold in both cases. Eighteen percent of men in the full sample were obese, 49% overweight, 32% normal and less than 1% underweight. As for women, 17% were obese, but less than 33% were overweight and 49% had a normal weight. Interestingly, while only 15% of men employed in all waves were obese, 21% of men retired in all waves were obese. The same pattern was found for women (the corresponding figures are 14% and 24%). This large gap is probably best explained by the fact that individuals employed in all waves are on average younger than individuals retired in all waves. However, it suggests that the 50-69-year-old undergo serious weight change around retirement age.

Additional descriptive statistics seem to corroborate this view : in the pooled sample – irrespective of retirement status –, 11% of individuals experienced a weight change (either gain or loss) of at least 10% between two subsequent waves of the survey. Seventeen percent switched from underweight or normal weight categories to overweight or obesity between two subsequent waves of the survey, while 8% of overweight or obese individuals switched back to a normal weight category during the same period. These figures give evidence of a high within-individual weight variation in our sample, suggesting that weight change among the elderly can be rapid. Interestingly, Figure 2.1 suggests that weight change is even more important among individuals having retired between waves. Figure 2.1 plots the distribution of weight change for individuals having retired across waves as well as the distribution of weight change for individuals continuously employed or retired in all waves, for men and women respectively. A simple look at each graph suggests that the distribution is flatter for individuals having retired across waves : the peak around zero – meaning no weight change – is indeed less clear-cut in both graphs. Although the distributions are not significantly different – neither for men nor for women – it suggests that individuals who retire experience weight change to a higher extent than individuals continuously employed or retired. Similarly, the proportion of individuals experiencing a weight change of at least 10% between two subsequent waves of the survey is higher among men and women having retired (11% and 14% respectively) than among men and women continuously employed or retired (9% and 12% respectively).[17]

Different sets of covariates are used, depending on the specification used (POLS, FE or FEIV models). We introduce age and age squared in all specifications to control properly for the age trend and to account for a potential non-linear effect of age on weight. Each specification also includes marital status (lives with a spouse-partner/does not live with a spouse-partner) and time dummies for 2006 and 2010. The average age of men and women in the pooled sample was 59.8 and 59.7 years old respectively. On average, men and women having retired between 2004 and 2010 were aged 60.3 and 60.4 years old respectively. Eighty-seven percent of men in the full sample lived with a spouse or partner, while only 72% of women did so. Gender, educational level[18] (primary education/lower secondary/upper secondary/post-secondary), occupation[19] (blue collars/white collars/technicians/managers and professionals) and country dummies are only included in the POLS specification, as FE and FEIV models do not permit to identify the effects of time-invariant variables. Summary statistics for gender, educational level, occupation and country can be found in Tables 2.2 and 2.3 for men and women respectively. Seventeen percent of men in the full sample had achieved primary education, 18% lower secondary education, 33% upper secondary education and 32% post-secondary education. The corresponding figures for women are 17%, 18%, 30% and 35%. Thirty-three percent of males in the pooled sample were in blue-collar occupations, 13% in white-collar occupations, 20% were technicians and 34% managers or professionals. Similarly, 20% of women in the full sample were in blue-collar occupations, 32% in white-collar occupations, 19% were technicians and 29% managers or professionals. Men and women having retired across waves exhibited the same patterns of education and occupation than individuals in the full sample. Belgium, Sweden, France, Italy and Germany were the most represented countries in the male and female pooled samples. Note that we do not include health variables in our specifications. This is because health status is co-determined with retirement as well as weight, and controlling for it is likely to generate some endogeneity in our models.

[17] This is only suggestive evidence, given that the two proportions are not statistically different according to the khi-square test (neither for men, nor for women).

[18] Based on the 1997 International Standard Classification of Education (ISCED 97)

[19] Based on the 1988 International Standard Classification of Occupations (ISCO 88). Occupation is not time-varying in our data. Given that we focus on elderly workers, it seems to be a plausible assumption.

Finally, we supplement our dataset by the ERA in force in each country at the time of the survey (see Table 2.1). We build a dummy variable for each individual indicating whether his age at time $t$ is above or below the ERA in force at time $t$ in his country.

## 2.4. Results

### 2.1. Determinants of retirement

Almost 23% (816 individuals) of the individuals working at baseline retired between 2004 and 2010. Among them, 45% (365 individuals) had reached the national ERA during the same period. This suggests that actual retirement behaviour is well correlated with the ERA.

First-stage results are reported in Table 2.4 for men and women respectively. As expected, they indicate that the ERA is an important predictor of retirement. Reaching the ERA increases the probability of retiring by 21 and 28 percentage points for men and women respectively (both effects are significant at the 1% level). These coefficients can also be interpreted as the proportions of compliers in our sample[20] (21% among men and 28% among women), which are high. These results, combined with F-stats of the excluded instrument of 122.2 and 169.4 for men and women respectively, show that reaching the ERA provides a strong exogeneous shock on retirement behaviour.

Once controlling for these country-specific age breaks, the probability of retiring decreases with age up to a certain point, where it increases again – probably when reaching the official retirement age. Finally, neither time dummies for 2006 and 2010 nor marital status appear to be statistically important for retirement behaviour.

### 2.2. The impact of retirement on BMI, overweight and obesity

Given the differences in terms of both biological constitutions and labour market histories, we run separate models for men and women. Tables 2.5 and 2.6 report the POLS estimates for the BMI (column 1), the probability of being either overweight or obese (column 2) and the probability of being obese (column 3) for men and women respectively. All specifications include age, age squared, time dummies for 2006 and 2010, marital status and time-invariant variables such

[20] This is true in our case because both $R_{it}$ and $Z_{ict}$ are dummy variables and because our model is estimated by fixed-effect two-stage least squares.

as education, occupation and country dummies. Most of the control variables are statistically significant and of the expected sign. A steep education gradient in BMI, overweight and obesity is found for women and to a lower extent for men. As compared with primary education, post-secondary education is indeed associated with a lower BMI and a lower probability of being either overweight or obese as well as being obese for both men and women. Once controlling for education, occupation is not significantly associated with BMI, overweight and obesity, except for women : females in managerial or professional occupations have a lower probability of being overweight than blue-collar females. Living with a spouse or partner does not seem to be correlated with BMI or the probability of being obese but is associated with a higher risk of being either overweight or obese among men. Most country indicators are significant.[21] Surprisingly enough, once we control for retirement behaviour, age has a small and insignificant impact on BMI, overweight and obesity.

Our baseline specification reveals a positive and significant association between retirement and weight outcomes for men as well as women. Retirement is positively correlated with BMI : it increases BMI by 0.50 and 0.69 units for men and women respectively (both effects are significant at the 5% level).[22] It also increases men's probability of being either overweight or obese and men's probability of being obese by 4.8 and 3.8 percentage points respectively (both effects are significant at the 10% level). These coefficients correspond to a 7% (resp. 22%) increase in the probability of being overweight or obese (resp. obese) for men (compared with the sample average). Retirement also increases women's probability of being obese by 5.2 percentage points (at the 5% significance level). This represents a 37% increase in the probability of being obese for women.

However, these correlations are hard to interpret, because they potentially reflect the effects of unobserved characteristics that may affect both weight outcomes and retirement behaviour. The importance of confounding factors is apparent when we look at the coefficents on retirement once implementing fixed-effect regressions (see Table 2.7 and Table 2.8 for men and women respectively). Once taken into account the potential endogeneity arising from the correlation between retirement and time-invariant unobserved characteristics, retirement is no longer significantly associated with weight outcomes for men. The sign of the coefficient even becomes

[21] Results not shown but available upon request. Note that our results are virtually unchanged when including country*time fixed effects, suggesting that the time trend in obesity is fairly common across countries.

[22] For an average man measuring 1.75m and weighing 82kg, it corresponds to a 1.5 kilo gain. As for an average woman measuring 1.63m and weighing 69kg, it corresponds to a 1.8 kilo gain.

negative for BMI and the probability of being either overweight or obese (although both effects are insignificant at conventional levels). Conversely, retirement leads to weight gain (by 0.25 BMI, at the 5% significance level) and increases the probability of being obese for women (at the 10% significance level), although the magnitude of the estimates declines as compared to POLS results. Not controlling for time-invariant factors – such as time preference for instance, which has a positive effect both on the probability of retiring and on weight gain – may indeed generate an upward bias and account for the larger effect of retirement on weight in POLS models.

However, the fixed-effect estimates cannot be interpreted as causal : a number of omitted time-varying factors can easily generate some biases in the results. Health or psychological deteriorations – for instance – may trigger both retirement and weight change. Hence, we need to take into account the remaining endogeneity in the model by instrumenting retirement behaviour. Results are presented in Tables 2.9 and 2.10 for men and women respectively. Under the hypothesis that reaching the ERA is a valid instrument, our preferred IV estimates show that retirement induced by discontinuous incentives in early retirement schemes does not significantly affect men's BMI nor men's probability of being either overweight or obese, although both coefficients are positive. It causes, however, a 13 percentage point increase in the probability of being obese (at the 5% level) within a two to four-year period among men.[23] It corresponds to a 60% increase in the probability of being obese within a two to four-year period.[24][25] At this point, it should be noted that our FEIV estimates identify a Local Average Treatment Effect (LATE) among a sub-population of compliers, i.e, the effect of retirement for those who effectively retire at country-specific ERAs. As the ERA is probably more binding for individuals with long careers, we expect compliers to be less educated people. By contrast, the

[23]Note that we find a significant impact of retirement on men's BMI if we restrict our sample to men who had a BMI between 25 and 30 at baseline. The coefficient associated with retirement is equal to 0.87 (standard error : 0.53) and significant at the 10% level. This result is quite consistent with the significant impact of retirement on men's probability of being obese.

[24]When choosing an alternative threshold for obesity, e.g. 31, we find that the impact of retirement on the probability of being obese is marginally significant (at the 15% level) and in the same range of magnitude (coefficient : 0.8, standard error : 0.05)

[25]The coefficients associated with the effect of retirement on BMI in FEIV models are very close to the ones obtained for the USA using a similar FEIV strategy. We find that retirement causes a 0.47 and 0.18 BMI increase within a two to four-year period among men and women respectively (although both coefficients are insignificant at conventional levels). These estimates are comparable to Chung et al. (2009) findings : on US data, retirement causes a 0.24 increase in BMI within a two-year period (at the 10% significance level). Unfortunately, as Chung et al. (2009) did not study the causal impact of retirement on the probability of being either overweight or obese nor on the probability of being obese, other comparisons based on the magnitude of the coefficients cannot be made.

fixed-effect model estimates the average effect of retirement for all those who retire during the period under study.

Overall, our results seem to suggest a non-linear impact of retirement on men's BMI : retirement would mostly affect the right-hand side of the BMI distribution, thus increasing the risk of obesity.

To inquire this further, we estimate the distribution of men's BMI under different treatments for the subpopulation of compliers, following Imbens and Rubin (1997b).[26] More specifically, we estimate the distribution of BMI standardised by age under different treatments for the subpopulation of compliers. Figure 2.2 plots the estimated distributions of BMI standardised by age for winning and losing compliers. In our setup, winning compliers are individuals who became eligible to early retirement schemes between two subsequent waves of the survey and *did* retire then – but who would not have retired if they had not become eligible; losing compliers are individuals whose eligibility to early retirement schemes did not change between two subsequent waves of the survey and *did not* retire then – but who would have retired if they had become eligible. According to Figure 2.2, the density function of winning compliers is shifted to the right compared to losing compliers. Winning compliers also seem to be more dispersed than losing compliers. Interestingly, the right tail of the winning compliers' density is fatter after threshold 1 – broadly corresponding to a BMI around 30 for all ages.[27] [28] This is evidence that obese individuals are more frequent among the winning compliers. This piece of graphical evidence is consistent with the FEIV results discussed above and the idea that retirement has a non-linear impact on men's BMI.

Overall, retirement seems to have a non-linear impact on men's BMI : it mostly affects the right-hand side of the BMI distribution and increases the risk of obesity. As for women, Table 2.10 shows that they do not experience weight changes following retirement. The coefficient associated with retirement (although positive) is never significant, whatever the outcome.

[26]We provide a brief explanation of this method in the appendix.

[27]For all ages between 50 and 69, the mean BMI among men is close 26.5 and the standard deviation close to 4.

[28]By looking at Figure 2.2, one may wonder why the distribution of the BMI standardised by age for losing compliers takes negative values in the [1.8; 3] range. This point is discussed by Imbens and Rubin (1997b). According to the authors, this negativity can be due either to sampling variation or to violation of the assumptions. In our case, as the density takes negative values when it is very close to zero and for a limited range of values, this negativity is most likely to be due to sampling variation.

## 2.3. Heterogeneous effects of retirement

The impact of retirement on weight outcomes is likely to be highly heterogeneous across job types. In particular, individuals having retired from physically-demanding jobs are likely to gain weight if they do not compensate the direct reduction in job-related exercise by increasing their leisure-time physical activity or by decreasing their food intake. In order to test for this, we re-run our FEIV models by adding an interaction term of retirement status with a measure of previous job's physical strenuousness.[29] The physical strenuousness of work is measured using a question asking workers their opinion about the following statement : "My job is physically demanding". Four answers are available ranging from "strongly agree" to "strongly disagree". We dichotomise the responses into strenuous work (strongly agree/agree) and sedentary work (disagree/strongly disagree). As this information is only available in SHARE for individuals who were working at baseline, FEIV models are estimated on a smaller sample – 934 men and 808 women across three waves. Among these individuals working at baseline, 56.4% had a sedentary job and 43.6% had a strenuous job. Table 2.11 shows the results when interacting retirement status with our indicator of job strenuousness. It reports the FEIV estimates for the BMI (columns 1 and 2), the probability of being either overweight or obese (columns 3 and 4) and the probability of being obese (columns 5 and 6) for the baseline specification only. The first column of each pair presents the results for men, while the second column presents the results for women. As shown in column (5), the retirement effect on obesity seems to be mainly driven by men having retired from strenuous jobs. The coefficient associated with retirement is equal to 0.16 and insignificant at conventional levels, but the interaction term is equal to 0.10 and significant at the 5% level. Both coefficients are jointly significant at the 5% level. Overall, retirement causes a 26 percentage point increase in the probability of being obese among men having retired from strenuous jobs within a two to four-year period (at the 5% significance level). However, it does not seem to have a significant impact on neither their BMI nor their probability of being either overweight or obese. Retiring from a sedentary job does not seem to affect men's weight outcomes. Overall, our results suggest that retiring from a strenuous job has a triggering effect on obesity for men. As for women, columns (2), (4) and (6) show that they do not experience weight changes following retirement, whether they have retired from

[29] We derive an additional instrument for this interaction term by interacting our instrument $Z_{ict}$ with our indicator of job's physical strenuousness. This is only valid if the strenuousness of job is exogeneous with respect to weight change. It might be the case that individuals gaining weight between two subsequent waves of the survey switch to less demanding occupations prior to retirement. However, given that employment perspectives and career mobility are low among the elderly, this might not happen very often.

strenuous or sedentary jobs. The coefficients associated with the retirement indicator and the interaction term are never significant, whatever the outcome.

The impact of retirement on weight outcomes is also likely to be highly heterogeneous across weight at baseline. Additional results show that the causal impact of retirement on the probability of being obese is only significant for men who already had a BMI higher than 24 at baseline – whether we estimate the model with the interaction term *retirement*job strenuousness* or not.[30] The "marginal" individual – the individual likely to become obese through retirement – is thus a man already at risk of obesity at baseline, i.e., already overweight or not far from the overweight threshold before retirement.

Overall, our results show that retirement effects can be highly heterogeneous across gender, previous occupational strenuousness and baseline weight category. In particular, our results show that retiring from a strenuous job while being at risk of obesity before retirement (having a BMI higher than 24 at baseline) has a triggering effect on obesity for men.

## 2.4. Underlying mechanisms

According to our results, retirement increases the probability of being obese among men, but has no effect on women's weight outcomes. This section further investigates this heterogeneous response to retirement according to gender. As retirement is likely to operate on weight through physical activity and food intake, we try to assess whether changes in food intake and physical activity following retirement are gender-specific.

As a first step, we focus on changes in leisure-time physical activity after retirement. Leisure-time physical activity is captured in SHARE by the following question : "How often do you

[30] When considering men who already had a BMI higher than 24 at baseline, our sample goes down to 1054 men across the three waves. We re-run our FEIV models on this subsample to estimate the effect of retirement on the probability of being obese. The coefficient associated with retirement is equal to 0.15 (standard error : 0.07) and significant the 5% level. The coefficient associated with retirement is insignificant on the subsample of men who had a BMI lower than 24 at baseline (299 men across the three waves). We also re-run our FEIV models including the interaction term *retirement status*job strenuousness*. When considering men who already had a BMI higher than 24 and who were working at baseline, our sample goes down to 721 men across the three waves. When estimating the effect of retirement on obesity, the coefficient associated with retirement is equal to 0.17 (standard error : 0.14) and insignificant at conventional levels. The coefficient associated with the interaction term is equal to 0.11 (standard error : 0.06) and significant at the 10% level. Both coefficients are insignificant on the subsample of men who had a BMI lower than 24 and who were working at baseline (213 men across the three waves).

engage in activities that require a moderate level of energy such as gardening, cleaning the car, or doing a walk?". Four answers are available ranging from "more than once a week" to "hardly ever, or never". We dichotomize the responses into high (more than once a week/once a week) and low leisure-time physical activity (one to three times a month/hardly ever, or never). When using this specific indicator, our FEIV models show that women tend to increase their leisure-time physical activity following retirement, while men do not. Our results imply that retirement causes a 14 percentage-point increase in the probability of performing a moderate physical activity at least once a week (at the 5% significance level) among women. The corresponding figure for men is equal to 7 percentage points and insignificant at conventional levels. This would be suggestive evidence that the heterogeneous impact of retirement across genders is partly explained by women's higher propensity to engage in leisure-time physical activity following retirement. However, when using alternative dichotomisations of leisure-time physical activity, our results show that both men and women tend to increase their physical activity following retirement.[31]

Overall, whether the heterogeneous impact of retirement across gender is explained by gender-specific patterns in leisure-time physical activity is not clear.

We then look at changes in food intake after retirement. SHARE contains two measures of food consumption : the monthly household expenditure on food consumed away from home and the monthly household expenditure on food consumed at home. These two measures, however, are hard to interpret, as they reflect a household joint decision concerning food consumption. They do not necessarily reflect an individual change in food consumption – and even less an individual change in food intake. Due to these data limitations, the results obtained have to be interpreted with caution. Quite interestingly though, we find that men tend to increase the amount of food consumed at home after retirement. Our FEIV models show that men increase by 170 euros (corresponding to a 30% increase) their monthly consumption of food consumed at home (p-value : 0.052). As for women, the coefficient associated with retirement is positive – a 35-euro-increase – but far from significant. When turning to the monthly household expenditure on food consumed away from home, we do not find any significant effect of retirement, neither for men, nor for women.

[31] In particular, when redefining the leisure-time physical activity variable (as hardly never or never versus more than once a week/once a week/one to three times a month), we find that retirement causes a 11 (12) percentage point increase in the probability of performing a moderate physical activity at least one to three times a month among men (women). Both coefficients are significant at the 5% significance level.

These results, however, are quite hard to interpret. As we do not have any information regarding the quality of food consumed away and at home, it is difficult to know whether this increase in men's expenditure on food consumed at home corresponds to a healthier diet (or conversely, to a more detrimental diet, e.g. by increasing snacking in-between meal times after retirement). A possible interpretation, however, can be found in the role of time constraint in food choice. According to Mancino (2003), time pressure or the need for convenience can be situational factors leading individuals to forgo good intentions (healthy eating) for more immediate gratification, e.g. through consuming food prepared away from home, or consuming prepared food. Welch et al. (2009) indeed show that time pressure is reported as a barrier to healthy eating by 41% of women in Australia. Cawley and Liu (2012) find that employed women – as compared to women not in the labour force – spend significantly less time cooking and are more likely to purchase prepared foods in the US. Now, retirement typically relieves the pressure of time constraints and lowers the opportunity cost of time. To this extent, there are good chances that men and women's response to it be different. Women typically spend more time cooking than men. When single, they consume less prepared food than single men – see Ricroch (2012) for empirical evidence in France. Because the time constraint in food choice was actually binding for them, women may respond to retirement by cooking more. Although they probably eat more often at home following retirement, their monthly household expenditure on food consumed at home may not change, as their consumption of prepared food (which is on average more expensive) is likely to decrease. As for men – who generally retire earlier than their wives and hence have to feed themselves following retirement –, they may not be that sensitive to the relief of the time constraint. Few of them actually cook – especially as we consider older cohorts. Consequently, they may respond to retirement by consuming more prepared food, which would explain the increase in the monetary amount of food consumed at home after retirement.

Overall, our data lead to inconclusive results as regards gender-specific patterns of food intake and leisure-time physical activity following retirement. It is not surprising, as only very precise measures of food intake and physical activity would have allowed us to investigate this matter in greater detail. For instance, whether women and men compensate the direct reduction in job-related exercise to a different extent by increasing leisure-time physical activity is difficult to study using only self-reported items of physical activity measured on a five-point scale. Thus, data limitations make it difficult to explore the underlying mechanisms through which retirement affects weight.

## 2.5. Robustness checks

Our estimation strategy is likely to yield unbiased results if properly controlling for the age trend. As one may worry that our results be driven by an inadequate estimation of the age effect, we have tried linear, quadratic (presented) and quartic age terms in robustness checks. Results are qualitatively similar.[32]

As underweight status is associated with a higher risk of morbidity and mortality for the elderly (Corrada et al. (2006)), one could be afraid that underweight individuals have a different response to retirement. It might be the case that underweight individuals lose weight because of retirement, thus leading to an overall insignificant impact of retirement on BMI. We check that our results are robust to the exclusion of underweight individuals by re-running our IV estimates on normal, overweight and obese individuals at baseline. Results are virtually unchanged.[33]

Until now, retirement has been defined using a question on self-declared current job situation (see Data section). According to this definition, anyone who declares herself to be retired is considered as retired. One concern could be that individuals declare themselves as retired even when working full or part-time, simply because they have left their “career” job. We use an alternative definition according to which anyone who declares herself as “retired” and who did not do any paid work during the month preceding the interview is considered as retired (see Data section). The point estimates obtained on the retirement indicator when using this alternative definition do not significantly vary as compared to those presented in Table 2.9. In particular, when considering the probability of being obese as an outcome, the coefficient associated with retirement in the FEIV model for men is equal to 0.13 (standard error : 0.09) and significant at the 15% level. Given that only 395 individuals retire between 2004 and 2010 according to this alternative definition, this result is likely to be due to a power problem.

An additional concern is that our model does not control for country-specific time trends (e.g.

[32] When introducing age as a linear term, the point estimate associated with the effect of retirement on the probability of being obese in FEIV models for men is very similar to the one obtained when introducing age as a quadratic term (presented). The coefficient associated with retirement is equal to 0.13 (standard error : 0.06) and significant at the 5% level. The corresponding figure when introducing age as a quartic term is 0.14 (standard error : 0.08), significant at the 10% level. We find no significant results for men’s BMI nor men’s probability of being either overweight or obese. No significant results are found for women.

[33] When considering the probability of being obese as the outcome in the FEIV model for men, the coefficient associated with retirement is equal to 0.13 (standard error : 0.06) and significant at the 5% level. When considering either the BMI or the probability of being obese as the outcome, the coefficient associated with the retirement indicator in FEIV models for men is still insignificant. No significant results are found for women.

differential trends in food supplies, health policies or early life conditions). If these country-specific trends cause a nonlinear relationship between weight and age at the country-specific ERAs, our model may not estimate the true effect of retirement on weight. Given that we consider country and time-varying ERAs as well as several cohorts, it seems highly unlikely. However, an imperfect way to test for this is to introduce the age*country and age$^2$*country terms in our FEIV models. By doing so, we test whether age has a differential impact on weight across countries. All coefficients associated with these additional terms are insignificant in our FEIV models. The point estimates obtained on the retirement indicator do not significantly vary as compared to those presented in Table 2.9. In particular, when considering the probability of being obese as an outcome, the coefficient associated with retirement in the FEIV model for men is equal to 0.14 (standard error : 0.15) and significant at the 10% level.

One may worry that our results may be driven by the particular strong effect of retirement on weight in a specific country. Our results, however, are virtually unchanged when dropping one country at a time from our sample. Similarly, one may worry that the reforms undertaken in Italy and Austria – i.e. the stepwise increase in ERAs between 2004 and 2011 – may lead to anticipatory behaviour, which would bias our results. Our results are unchanged when excluding Italy and Austria from our sample.

Finally, we conduct a placebo test to back the reliance of our results. We evaluate the impact of retirement in a fictive state of the world where ERAs would be interchanged across countries.[34] We re-run our FEIV regressions with this fictive instrument. As expected, the coefficient associated with this fictive instrument in the first stage is close to 0 and insignificant at conventional levels. The F-stat of the excluded instrument is equal to 2.24 – below the standard requirement of 10 (Bound et al. (1995)) – thus suggesting a weak instrument problem. As expected, we do not find significant effects of retirement on weight outcomes in the second stage.

In this paragraph, we run additional robustness checks. In particular, we check that our FEIV results are robust to the presence of serial correlation in the error terms. We also consider an unbalanced panel and alternative estimation strategies.

If the error terms in the FEIV model were serially correlated, the usual standard errors obtained from it could be very misleading. We re-run our FEIV models allowing for clusters at

[34] The design of the placebo reform is as follows. We interchange ERAs across countries, e.g. we assign to each country an ERA in force in another country of the sample. France's ERA is set to 61. The corresponding ERAs for Germany, Sweden, Switzerland, Spain, Austria, Italy and Belgium are 57, 60, 60, 63, 59, 62 and 62, respectively.

the individual level. Results are virtually unchanged.[35]

As mentioned earlier, attrition rates are non-negligible in SHARE. In our setup, high attrition rates are a concern if non-response is systematically related to weight. We deal with panel attrition by using the approach developed by Becketti et al. (1988) and Fitzgerald et al. (1998). This approach is based on the assumption that all determinants of attrition can be controlled for (selection on observables). In the test, the value of the BMI at the initial wave of the survey is regressed on future attrition A (i.e. whether the individual later attrites). The test for attrition selection is simply based upon the significance of A in that model. The results (available upon request) indicate that A is not significant in that model, suggesting that people with higher BMI are no less likely to participate in further waves of SHARE. This is evidence of the absence of attrition bias due to weight. As an additional robustness check, we re-run our FEIV models on an unbalanced sample[36] to back the reliance of our results. The point estimate obtained on the retirement indicator when considering the probability of being obese as the outcome does not significantly vary as compared to the one presented in Table 2.9 : it is equal to 0.08 (standard error : 0.05) and significant at the 10% level.

Finally, we check that our results are robust to alternative estimation strategies. More specifically, we consider a pooled-IV model. If our instrument $Z_{ict}$ was truly exogeneous – i.e., if it was uncorrelated with the error term $u_{it}$ in equation (1) – the results obtained in the pooled-IV model should not be markedly different from the FEIV model. We run a two-stage least squares (2SLS) model where the retirement status is instrumented by the dummy indicator $Z_{ict}$. The covariates included in the model are, as usual, age, age squared, marital status, occupation, education, as well as time and country dummies. Standard errors are robust and clustered at the individual level. We find no significant results neither for men's BMI, nor for men's probability of being either overweight or obese. As for the probability of being obese, the coefficient associated with the retirement indicator is in the same range of magnitude than the one obtained in the FEIV model : it is equal to 0.12 (standard error : 0.15) but not significant at conventional levels. No significant results are found for women. We further investigate the impact of retirement on the probability of being obese in the pooled-IV setting by implementing a bivariate probit with $Z_{ict}$ as an identifying variable. Both the 2SLS and the bivariate models are consis-

[35] In particular, when considering the probability of being obese as the outcome in the FEIV model for men and when clustering at the individual level, the coefficient associated with retirement is equal to 0.12 (standard error : 0.06) and significant at the 5% level.

[36] Once conditioning on having no missing values on weight, height and any covariate included in the model and having at least two observations per individual across the three waves, the unbalanced sample consists of 18,199 observations in eight countries (9,693 men and 8,506 women).

tent, but only the bivariate model is efficient in our case, as both endogeneous variables (the retirement and the obesity indicator) are dichotomous. The marginal effect associated with the retirement indicator after implementing the endogeneous bivariate probit is equal to 0.10 and significant at the 10% level. Overall, our results seem to be robust to alternative estimation strategies and confirm the positive and significant impact of retirement on the risk of obesity. The estimated impact of retirement on the probability of being obese always lies in the same range of magnitude, between 0.10 and 0.13.

## 2.5. Conclusion

This paper studies the effect of retirement on several weight outcomes using the 2004, 2006 and 2010 waves of SHARE. It exploits the European variation in ERAs and the stepwise increase in ERAs in Austria and Italy during the period under study to produce an exogeneous shock on retirement behaviour. This allows us to estimate the short-term causal impact of retirement on weight. Our results show that retirement induced by social security rules causes a 13 percentage point increase in the probability of being obese within a two to four-year period among 50-69 year-old men. Our findings suggest that retirement has a non-linear impact on men's BMI, mostly affecting the right-hand side of the BMI distribution. We give evidence that this effect is highly heterogeneous and driven by men having retired from strenuous jobs who were already at risk of obesity. No significant effects are found among women.

A straightforward interpretation of our findings is that the impact of retirement among men having retired from strenuous jobs is driven by a direct reduction in job-related exercise. However, an alternative interpretation would be that these men also share social norms that shape their response to retirement in terms of food intake, leisure-time physical activity or mental health. In our view, these two interpretations are highly complementary and both explain the higher risk of obesity faced by men having retired from strenuous jobs. Another interpretation of our results has to do with reporting bias in BMI : men and women tend to underestimate their weight in surveys (Niedhammer et al. (2000); Gorber et al. (2007)). Yet, if retirement is associated with a higher propensity to go to the doctor, new retirees are likely to have their weight measured by a physician following retirement and may acquire an accurate knowledge of their "true" weight. When interviewed in subsequent waves of the survey, they may adjust their self-reported weight and thus declare a higher weight. In this context, the impact of retirement on self-reported BMI would result rather from a decline in the reporting bias in weight than from a true increase in BMI. However, this interpretation relies on several assumptions which do not necessarily hold. First, there is no evidence that retirement increases the use of medical care.[37] Second, there is no evidence that misreporting bias in weight results from a lack of knowledge; there is no evidence either that individuals adjust their self-reported weights when obtaining accurate knowledge about it (Niedhammer et al. (2000)).[38]

Interestingly enough, we find that women's weight outcomes are not affected by retirement. There is some evidence in the literature that women adjust to retirement more successfully than men (Barnes and Parry (2004)). Women may adjust their food diet and physical activity to a better extent than men. Due to data limitations, we were not able to investigate this question in greater detail. However, a promising avenue for future research would consist in investigating gender-specific responses to retirement, especially in terms of food intake and physical activity.

Health is multidimensional; its dimensions can be diversely affected by retirement. If retiring reduces the amount of stress and physical strain, it may improve subjective measures of health (self-rated health, mental health or well-being). If, at the same time, retirement reduces the amount of physical activity and mentally stimulating activities an individual experiences from work, it may deteriorate objective ones (cognitive or cardiovascular functioning for instance). A number of papers in the literature seem to support this idea.[39] Our results are highly consistent with this interpretation : although declared, the BMI can be seen as an objective measure of health. The direct reduction in job-related exercise following retirement is likely to deteriorate this specific dimension of health, along with other dimensions of objective health.

A limitation to this study is that we only consider the traditional and more frequent pattern of retirement, where individuals transit directly from work to retirement. We do not consider more complex pathways to retirement (via unemployment, disability or inactivity). This sample selection implies that our results do not necessarily generalise to other transitions to retirement. Further research would be needed to get a fuller picture of the impact of different patterns of

[37] On the contrary, Fe and Hollingsworth (2012) find that retirement decreases primary care use in the UK.

[38] In a study on the validity of self-reported weight and height in the French GAZEL cohort, Niedhammer et al. (2000) provide convincing evidence that "the reporting bias in BMI results more from inexact reporting of weight and height that are accurately known than from lack of knowledge".

[39] Although some findings in the literature are not consistent with this interpretation. See for instance Dave et al. (2006), who find negative effects of retirement on both objective and subjective measures on health or Coe and Zamarro (2011), who find positive effects on both objective and subjective measures on health.

retirement on subsequent weight and health changes.

In a context where half of OECD countries are increasing retirement ages or will do so in the coming decades, an important policy question is whether retiring at older ages have a stronger impact on weight. As we have country and time-varying ERAs, we investigate this further by allowing for different retirement effects depending on the age at which an individual is entitled to retire (below or above age 60). The results obtained are to be interpreted with caution, as the standard errors in our model are rather large and coefficients are not significant at conventional levels. However, our results suggest an age-gradient story : men retiring after age 60 when their country allows for retirement have a 0.5 percentage point higher probability of being obese than men retiring before age 60.

Our results have some important policy implications. Given the increasing number of people approaching retirement age and the upward trend in obesity rates (where each cohort is heavier than the previous one), men already at risk of obesity and retiring from strenuous jobs will be likely to suffer from health disorders in the near future – especially as obesity is a major risk factor for cardiovascular diseases among men in their sixties. From an inequality perspective, the heterogeneous impact of retirement may exacerbate weight and health disparities, as retirement seems to affect the most vulnerable individuals (men in strenuous jobs and at risk of obesity). Public health policies specifically targeted at this population should be considered in order to guarantee healthy ageing and healthy life years following retirement.

# Tables and Figures

**Figure 2.1** – Distribution of weight change (in kg) among individuals having retired across waves and individuals either employed or retired in all waves, for men and women respectively.

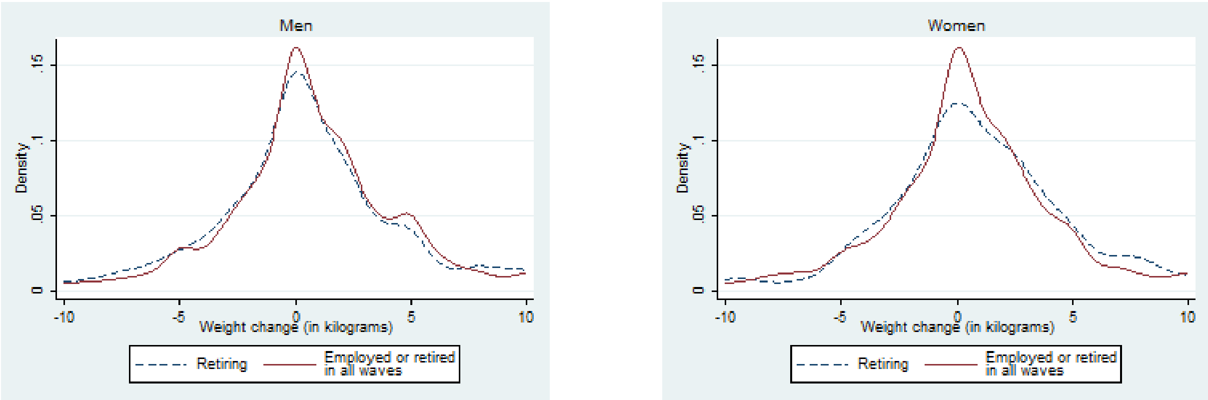



**Figure 2.2** – Counterfactual distributions of men's BMI standardised by age for the subpopulation of compliers.

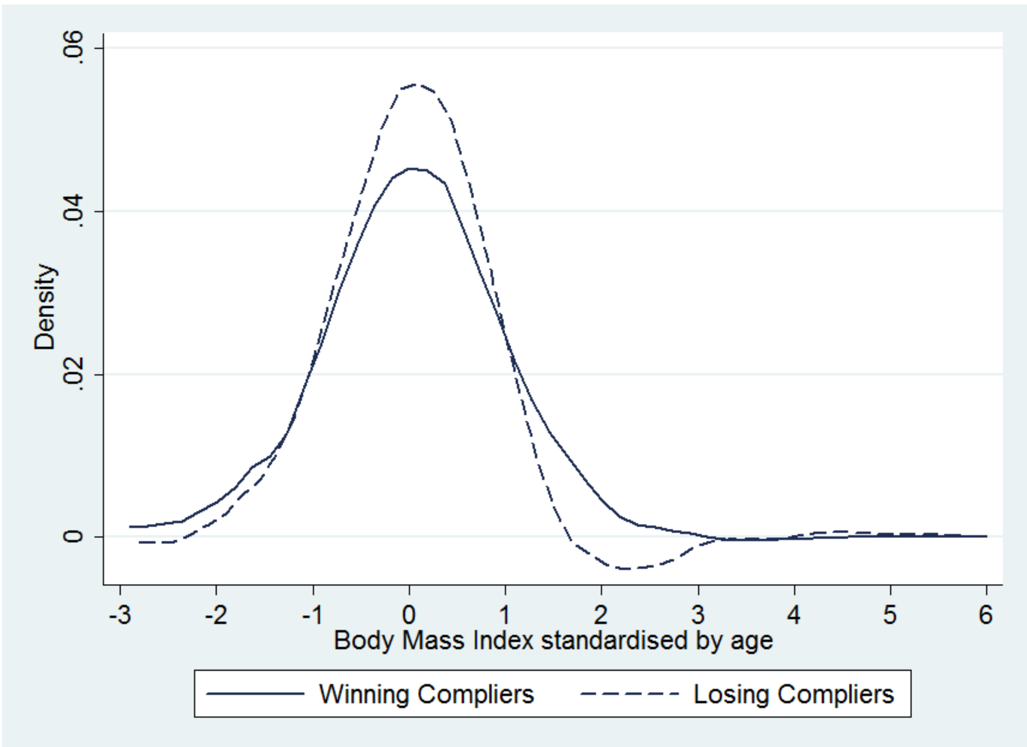


**Table 2.1** – Official (ORA), Earliest (ERA) and Effective retirement ages; Proportion of individuals retired below and above the ERA cutoff and proportion of individuals retiring when reaching the ERA between two subsequent waves of the survey.

| Country | Official retirement ages (ORA)[a] | | Earliest retirement ages (ERA)[a] | | Effective retirement ages[d,e] | | % of retired below ERA[d] | % of retired above ERA[d] | % of individuals retiring when reaching ERA across waves[f] |
|---|---|---|---|---|---|---|---|---|---|
| | Men (1) | Women (2) | Men (3) | Women (4) | Men (5) | Women (6) | (7) | (8) | (9) |
| Austria | 65 | 60 | 61.5[b] | 56.5[b] | 61.3 | 59.5 | 31.4 | 94.2 | 28.6 |
| Belgium | 65 | 63 | 60 | 60 | 61.4 | 61.4 | 21.1 | 81.6 | 34.3 |
| France | 65 | 65 | 60 | 60 | 60.5 | 62.1 | 17.0 | 88.0 | 44.5 |
| Germany | 65 | 65 | 63 | 60 | 63.6 | 62.6 | 11.5 | 79.9 | 38.6 |
| Italy | 65 | 60 | 57[c] | 57[c] | 61.4 | 61.4 | 18.6 | 81.6 | 27.2 |
| Spain | 65 | 65 | 61 | 61 | 65 | 63 | 8.9 | 68.9 | 21.6 |
| Sweden | 65 | 65 | 61 | 61 | 65.5 | 65.9 | 6.5 | 50.7 | 19.4 |
| Switzerland | 65 | 64 | 63 | 62 | 65 | 65 | 5.4 | 65.4 | 39.0 |

[a] Official and earliest retirement ages are provided by Keese (2006) and OECD (2011) reports. They concern workers retiring in 2005 under the main mandatory pension schemes and exclude special arrangements for public-sector workers and other workers such as the long-term unemployed or disabled.
[b] In 2004, workers in Austria could retire at ages 61.5 (men) and 56.5 (women). The 2004 pension reform in Austria introduced a gradual increase in the ERAs for men and women. The ERAs were increased by two months for each quarter of birth for men born in the first two quarters of 1943 and women born in the first two quarters of 1948. Following these increases, the ERAs were increased by one month for each quarter of birth for men born in the third quarter of 1943 and later and for women born in the third quarter of 1948 and later. Furthermore, the 2004 pension reform also created special corridor pensions for men born in the last quarter of 1943 and later. The minimum entry age for these corridor pensions was 62, thereby making the ERA beyond age 62 non-binding in many cases (Manoli and Weber (2012)). Greater details about the reform can be found in Manoli and Weber (2012). We assign to each individual living in Austria the ERA corresponding to his quarter of birth and sex. We take age 62 as the binding age for men for the 2006 and 2010 waves.
[c] Before 2008, workers in Italy could retire at age 57 if they had contributed to the system for 35 years. According to a recent reform, approved as part of the 2008 budget process, the minimum age to request early retirement in Italy has increased from 57 to 61 years old in 2013. The minimum age to request early retirement in Italy was 59 years old from July 1, 2009 to December 31, 2010 and 60 years old from January 1, 2011 to December 31, 2012 (OECD (2011)). We thus consider age 57 as the ERA in force in Italy when waves 1 and 2 of SHARE were conducted on the field. As almost all the individuals of the 2010-2011 wave of SHARE were surveyed in 2011, we take age 60 as the ERA in force when wave 3 of the SHARE survey was conducted on the field.
[d] Figures in columns 5-8 are computed using the pooled sample, i.e, 7479 observations. We do not use the panel structure of the data to compute these estimates.
[e] We compute the effective retirement age as the average age of individuals who retired between 2004 and 2006 or between 2006 and 2010 in our data. As we do not have reliable information on the month and year in which the individuals retire, we cannot give the actual average age at which they retire. For this reason, figures in column 5-6 can be misleading because they systematically over-estimate the effective retirement age, which is calculated in 2006 for individuals having retired between 2004 and 2006 and calculated in 2010-11 for individuals having retired between 2006 and 2010-11.
[f] The panel structure of our data allows us to compute the proportion of individuals actually retiring between two subsequent waves of the survey when reaching the ERA in force in their country during the same period.

**Table 2.2** – Summary statistics for the pooled sample of men.

| **Characteristics** | | **Whole sample** | **Employed in all waves** | **Retired in all waves** | **Retiring between waves**[a] |
|---|---|---|---|---|---|
| | | Average | Average | Average | Average |
| | | (1) | (2) | (3) | (4) |
| *Demographics* | | | | | |
| Age | | 59.82 | 56.84 | 62.87 | 60.32 |
| *Marital status* | Lives with spouse/partner | 0.87 | 0.85 | 0.88 | 0.90 |
| | Doesn't live with spouse/partner | 0.13 | 0.15 | 0.12 | 0.10 |
| *Education* | Post-secondary | 0.32 | 0.41 | 0.22 | 0.32 |
| | Upper secondary | 0.33 | 0.29 | 0.33 | 0.37 |
| | Lower secondary | 0.18 | 0.18 | 0.18 | 0.17 |
| | Primary education | 0.17 | 0.11 | 0.27 | 0.15 |
| *Occupation* | Managers and professionals | 0.34 | 0.43 | 0.24 | 0.32 |
| | Technicians | 0.20 | 0.17 | 0.20 | 0.21 |
| | White collars | 0.13 | 0.12 | 0.15 | 0.13 |
| | Blue collars | 0.33 | 0.27 | 0.41 | 0.33 |
| *Employment* | | | | | |
| Retirement status | Retired | 0.45 | 0.00 | 1.00 | . |
| | Employed or self-employed | 0.55 | 1.00 | 0.00 | . |
| *Weight related measures* | | | | | |
| *Weight category* | Underweight | 0.01 | 0.00 | 0.01 | 0.01 |
| | Normal | 0.32 | 0.36 | 0.30 | 0.30 |
| | Overweight | 0.49 | 0.48 | 0.50 | 0.50 |
| | Obese | 0.18 | 0.15 | 0.21 | 0.19 |
| Body Mass Index | | 26.95 | 26.49 | 27.38 | 27.08 |
| *Country* | Austria | 0.08 | 0.05 | 0.12 | 0.08 |
| | Belgium | 0.22 | 0.19 | 0.24 | 0.24 |
| | France | 0.15 | 0.13 | 0.19 | 0.15 |
| | Germany | 0.09 | 0.11 | 0.06 | 0.09 |
| | Italy | 0.16 | 0.09 | 0.27 | 0.12 |
| | Spain | 0.07 | 0.08 | 0.06 | 0.08 |
| | Sweden | 0.16 | 0.24 | 0.04 | 0.17 |
| | Switzerland | 0.07 | 0.11 | 0.02 | 0.07 |
| Observations | | 4059 | 1497 | 1245 | 1317 |

[a] An individual retiring between waves is defined as an individual having retired either between 2004 and 2006 or between 2004 and 2006.

**Table 2.3** – Summary statistics for the pooled sample of women.

| **Characteristics** | | **Whole sample** | **Employed in all waves** | **Retired in all waves** | **Retiring between waves**[a] |
|---|---|---|---|---|---|
| | | Average | Average | Average | Average |
| | | (1) | (2) | (3) | (4) |
| *Demographics* | | | | | |
| Age | | 59.68 | 56.45 | 63.06 | 60.41 |
| *Marital status* | Lives with spouse/partner | 0.72 | 0.73 | 0.69 | 0.75 |
| | Doesn't live with spouse/partner | 0.28 | 0.27 | 0.31 | 0.25 |
| *Education* | Post-secondary | 0.35 | 0.42 | 0.25 | 0.37 |
| | Upper secondary | 0.30 | 0.30 | 0.29 | 0.29 |
| | Lower secondary | 0.18 | 0.17 | 0.19 | 0.19 |
| | Primary education | 0.17 | 0.11 | 0.27 | 0.15 |
| *Occupation* | Managers and professionals | 0.29 | 0.34 | 0.24 | 0.28 |
| | Technicians | 0.19 | 0.19 | 0.16 | 0.23 |
| | White collars | 0.32 | 0.32 | 0.34 | 0.30 |
| | Blue collars | 0.20 | 0.15 | 0.26 | 0.19 |
| *Employment* | | | | | |
| Retirement status | Retired | 0.43 | 0.00 | 1.00 | . |
| | Employed or self-employed | 0.57 | 1.00 | 0.00 | . |
| *Weight related measures* | | | | | |
| *Weight category* | Underweight | 0.01 | 0.01 | 0.01 | 0.02 |
| | Normal | 0.49 | 0.53 | 0.38 | 0.53 |
| | Overweight | 0.33 | 0.30 | 0.37 | 0.31 |
| | Obese | 0.17 | 0.14 | 0.24 | 0.14 |
| Body Mass Index | | 25.79 | 25.43 | 26.89 | 25.22 |
| *Country* | Austria | 0.07 | 0.02 | 0.15 | 0.06 |
| | Belgium | 0.20 | 0.19 | 0.24 | 0.17 |
| | France | 0.16 | 0.14 | 0.15 | 0.20 |
| | Germany | 0.12 | 0.14 | 0.09 | 0.13 |
| | Italy | 0.13 | 0.07 | 0.24 | 0.11 |
| | Spain | 0.04 | 0.06 | 0.01 | 0.03 |
| | Sweden | 0.21 | 0.27 | 0.10 | 0.24 |
| | Switzerland | 0.07 | 0.11 | 0.02 | 0.06 |
| Observations | | 3420 | 1299 | 990 | 1131 |

[a] An individual retiring between waves is defined as an individual having retired either between 2004 and 2006 or between 2004 and 2006.

**Table 2.4** – First-stage results : Impact of reaching the Earliest Retirement Age (ERA) on retirement status.

| | Retired | |
|---|---|---|
| | Men | Women |
| | (1) | (2) |
| Above the ERA | .209*** | .277*** |
| | (.019) | (.021) |
| Age | -.054* | -.142**** |
| | (.031) | (.031) |
| Age squared | .000** | .001*** |
| | (.000) | (.000) |
| Time dummy for 2006 | .049 | .040 |
| | (.045) | (.046) |
| Time dummy for 2010 | .188 | .151 |
| | (.129) | (.134) |
| Lives with spouse/partner | .012 | .021 |
| | (.040) | (.037) |
| R-squared | 0.30 | 0.34 |
| F-Stat of excluded instruments | 122.18 | 169.36 |
| Observations | 4059 | 3420 |

Notes : (1) *** : significant at the 1% level, ** : significant at the 5% level, * : significant at the 10% level. (2) Standard errors in parentheses are robust. (3) Columns 1-2 are estimated by fixed-effect linear probability models.

**Table 2.5** – Pooled OLS results for men : the impact of retirement on BMI, the probability of being either overweight or obese and the probability of being obese.

| | Men | | |
|---|---|---|---|
| | BMI | Overweight or Obese (BMI≥25) | Obese (BMI≥30) |
| | (1) | (2) | (3) |
| Retirement | .499** | .048* | .038* |
| | (.223) | (.025) | (.022) |
| Age | .152 | .053 | -.003$^{\mu}$ |
| | (.273) | (.034) | (.028) |
| Age squared | -.002 | -.001* | .000 |
| | (.002) | (.000) | (.000) |
| Time dummy for 2006 | .159** | .018$^{\mu}$ | .032*** |
| | (.081) | (.012) | (.009) |
| Time dummy for 2010 | .544*** | .065*** | .050** |
| | (.193) | (.023) | (.019) |
| *Marital status* | | | |
| *(Ref : Does not live with a spouse/partner)* | | | |
| Lives with spouse/partner | .217 | .072** | .009 |
| | (.323) | (.035) | (.028) |
| *Education (Ref : Primary education)* | | | |
| Post secondary education | -1.378*** | -.133*** | -.113*** |
| | (.356) | (.040) | (.035) |
| Upper secondary education | -.606* | -.069* | -.050$^{\mu}$ |
| | (.347) | (.036) | (.033) |
| Lower secondary education | -.634* | -.028 | -.043 |
| | (.351) | (.037) | (.034) |
| *Occupation (Ref : Blue collars)* | | | |
| Managers and professionals | .002 | .015 | -.021 |
| | (.290) | (.033) | (.028) |
| Technicians | .391 | .054$^{\mu}$ | .013 |
| | (.332) | (.034) | (.031) |
| White collars | .365 | .033 | .042 |
| | (.337) | (.035) | (.034) |
| Country fixed-effects | yes | yes | yes |
| R-squared | 0.04 | 0.04 | 0.03 |
| Observations | 4059 | 4059 | 4059 |

Notes : (1) *** : significant at the 1% level, ** : significant at the 5% level, * : significant at the 10% level, $^{\mu}$ : significant at the 15% level. (2) Standard errors in parentheses are clustered at the individual level. (3) Columns 2-3 are estimated by linear probability models.

**Table 2.6** – Pooled OLS results for women : the impact of retirement on BMI, the probability of being either overweight or obese and the probability of being obese.

| | Women | | |
|---|---|---|---|
| | BMI | Overweight or Obese (BMI≥25) | Obese (BMI≥30) |
| | (1) | (2) | (3) |
| Retirement | .691** | .048$^{\mu}$ | .052** |
| | (.301) | (.030) | (.023) |
| Age | -.060 | -.025 | .007 |
| | (.381) | (.038) | (.030) |
| Age squared | .000 | .000 | -.000 |
| | (.003) | (.000) | (.000) |
| Time dummy for 2006 | .272*** | .023* | .019* |
| | (.034) | (.013) | (.010) |
| Time dummy for 2010 | .164 | .005 | .012 |
| | (.067) | (.026) | (.019) |
| *Marital status* | | | |
| *(Ref : Does not live with a spouse/partner)* | | | |
| Lives with spouse/partner | .248 | .008 | -.008 |
| | (.276) | (.029) | (.022) |
| *Education (Ref : Primary education)* | | | |
| Post secondary education | -2.544*** | -.179*** | -.160*** |
| | (.542) | (.053) | (.042) |
| Upper secondary education | -1.653*** | -.133*** | -.113*** |
| | (.513) | (.048) | (.040) |
| Lower secondary education | -1.094** | -.066 | |
| -.097** | | | |
| | (.513) | (.047) | (.041) |
| *Occupation (Ref : Blue collars)* | | | |
| Managers and professionals | -.475 | -.133*** | -.033 |
| | (.499) | (.051) | (.039) |
| Technicians | .181 | -.044 | -.012 |
| | (.501) | (.050) | (.041) |
| White collars | -.487 | -.070* | -.026 |
| | (.433) | (.042) | (.035) |
| Country fixed-effects | yes | yes | yes |
| R-squared | 0.06 | 0.06 | 0.04 |
| Observations | 3420 | 3420 | 3420 |

Notes : (1) *** : significant at the 1% level, ** : significant at the 5% level, * : significant at the 10% level, $^{\mu}$ : significant at the 15% level. (2) Standard errors in parentheses are clustered at the individual level. (3) Columns 2-3 are estimated by linear probability models.

**Table 2.7** – Fixed-effects results for men : the impact of retirement on BMI, the probability of being either overweight or obese and the probability of being obese.

| | Men | | |
|---|---|---|---|
| | BMI | Overweight or Obese (BMI≥25) | Obese (BMI≥30) |
| | (1) | (2) | (3) |
| Retirement | -.122 | -.002 | .020 |
| | (.107) | (.019) | (.014) |
| Age | .358* | .089*** | .009 |
| | (.188) | (.032) | (.021) |
| Age squared | -.002 | -.001*** | .000 |
| | (.001) | (.000) | (.000) |
| Time dummy for 2006 | -.201 | -.010 | -.011 |
| | (.214) | (.041) | (.027) |
| Time dummy for 2010 | -.482 | -.011 | -.078 |
| | (.605) | (.119) | (.080) |
| *Marital status* | | | |
| *(Ref : Does not live with a spouse/partner)* | | | |
| Lives with spouse-partner | -.214 | -.064 | -.013 |
| | (.208) | (.043) | (.030) |
| R-squared | 0.91 | 0.80 | 0.83 |
| Observations | 4059 | 4059 | 4059 |

Notes : (1) *** : significant at the 1% level, ** : significant at the 5% level, * : significant at the 10% level. (2) Standard errors in parentheses are robust. (3) Columns 2-3 are estimated by fixed-effect linear probability models.

**Table 2.8** – Fixed-effects results for women : the impact of retirement on BMI, the probability of being either overweight or obese and the probability of being obese.

| | Women | | |
|---|---|---|---|
| | BMI | Overweight or Obese (BMI≥25) | Obese (BMI≥30) |
| | (1) | (2) | (3) |
| Retirement | .251** | .008 | .024* |
| | (.100) | (.017) | (.014) |
| Age | .175 | .014 | -.018 |
| | (.188) | (.032) | (.022) |
| Age squared | -.002 | .000 | .000 |
| | (.001) | (.000) | (.000) |
| Time dummy for 2006 | .367 | .061 | .035 |
| | (.271) | (.043) | (.032) |
| Time dummy for 2010 | .544 | .125 | .059 |
| | (.785) | (.127) | (.096) |
| *Marital status* | | | |
| *(Ref : Does not live with a spouse-partner)* | | | |
| Lives with spouse/partner | .439* | .022 | -.002 |
| | (.230) | (.032) | (.032) |
| R-squared | 0.93 | 0.86 | 0.84 |
| Observations | 3420 | 3420 | 3420 |

Notes : (1) *** : significant at the 1% level, ** : significant at the 5% level, * : significant at the 10% level. (2) Standard errors in parentheses are robust. (3) Columns 2-3 are estimated by fixed-effect linear probability models.

**Table 2.9** – Second-stage results for men : the causal impact of retirement on BMI, the probability of being either overweight or obese and the probability of being obese.

| | Men | | |
|---|---|---|---|
| | BMI | Overweight or Obese (BMI≥25) | Obese (BMI≥30) |
| | (1) | (2) | (3) |
| Retirement | .474 | .057 | .129** |
| | (.447) | (.073) | (.060) |
| Age | .407** | .094*** | .018 |
| | (.198) | (.032) | (.022) |
| Age squared | -.002* | -.001*** | .000 |
| | (.001) | (.000) | (.000) |
| Time dummy for 2006 | -.245 | -.014 | -.019 |
| | (.237) | (.042) | (.029) |
| Time dummy for 2010 | -.624 | -.025 | -.104 |
| | (.676) | (.121) | (.085) |
| *Marital status* | | | |
| *(Ref : Does not live with a spouse/partner)* | | | |
| Lives with spouse/partner | -.226 | -.065 | -.015 |
| | (.206) | (.041) | (.030) |
| Observations | 4059 | 4059 | 4059 |

Notes : (1) *** : significant at the 1% level, ** : significant at the 5% level, * : significant at the 10% level. (2) Standard errors in parentheses are robust. (3) Columns 2-3 are estimated by FEIV linear probability models. (4) As the **xtivreg2** command in Stata (Schaffer (2010)) only computes the whithin R-squared, the overall R-squared is not reported here.

**Table 2.10** – Second-stage results for women : the causal impact of retirement on BMI, the probability of being either overweight or obese and the probability of being obese.

| | Women | | |
|---|---|---|---|
| | BMI | Overweight or Obese (BMI≥25) | Obese (BMI≥30) |
| | (1) | (2) | (3) |
| Retirement | .176 | .022 | .014 |
| | (.360) | (.057) | (.044) |
| Age | .167 | .016 | -.019 |
| | (.193) | (.031) | (.024) |
| Age squared | -.002 | .000 | .000 |
| | (.001) | (.000) | (.000) |
| Time dummy for 2006 | .370 | .061 | .036 |
| | (.286) | (.042) | (.033) |
| Time dummy for 2010 | .553 | .124 | .060 |
| | (.823) | (.124) | (.098) |
| *Marital status* | | | |
| *(Ref : Does not live with a spouse/partner)* | | | |
| Lives with spouse/partner | .440** | .021 | -.002 |
| | (.216) | (.030) | (.030) |
| Observations | 3420 | 3420 | 3420 |

Notes : (1) *** : significant at the 1% level, ** : significant at the 5% level, * : significant at the 10% level. (2) Standard errors in parentheses are robust. (3) Columns 2-3 are estimated by FEIV linear probability models. (4) As the **xtivreg2** command in Stata (Schaffer (2010)) only computes the whithin R-squared, the overall R-squared is not reported here.

**Table 2.11** – Second-stage results for men and women : the impact of retirement by occupation type (strenuous/sedentary) before retirement.

| | BMI | | Overweight or Obese (BMI≥25) | | Obese (BMI≥30) | |
|---|---|---|---|---|---|---|
| | Men | Women | Men | Women | Men | Women |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Retirement | .419 | .733 | .050 | .085 | .162 | .088 |
| | (.894) | (.734) | (.139) | (.126) | (.116) | (.090) |
| Retirement*strenuous occupation | .461 | .033 | .012 | -.078 | .104** | -.037 |
| | (.371) | (.360) | (.058) | (.064) | (.053) | (.045) |
| Age | .728 | .552 | .112 | .030 | .127 | .024 |
| | (.739) | (.620) | (.102) | (.112) | (.087) | (.076) |
| Age squared | -.005 | -.005 | -.001 | .000 | -.001 | .000 |
| | (.006) | (.005) | (.001) | (.001) | (.001) | (.001) |
| Time dummy for 2006 | -.431 | .360 | -.016 | .070 | -.058 | .017 |
| | (.441) | (.389) | (.065) | (.059) | (.050) | (.042) |
| Time dummy for 2010 | -.935 | .492 | -.026 | .154 | -.222 | -.011 |
| | (1.21) | (1.08) | (.181) | (.168) | (.140) | (.121) |
| *Marital status* | | | | | | |
| *(Ref : Does not live with a spouse/partner)* | | | | | | |
| Lives with spouse/partner | 0.009 | .524** | -.037 | -.021 | .012 | .003 |
| | (.261) | (.236) | (.054) | (.032) | (.039) | (.036) |
| Observations | 2802 | 2424 | 2802 | 2424 | 2802 | 2424 |

Notes : (1) *** : significant at the 1% level, ** : significant at the 5% level, * : significant at the 10% level. (2) Standard errors in parentheses are robust. (3) Columns 3-6 are estimated by FEIV linear probability models. (4) Information on the physical strenuousness of work before retirement is only available for individuals who were working at baseline, i.e, 2802 men and 2424 women in the pooled sample. (5) As the **xtivreg2** command in Stata (Schaffer (2010)) only computes the whithin R-squared, the overall R-squared is not reported here.

# Appendix

## A-2.1. Estimating Outcome Distributions for Compliers in Instrumental Variables (IV) Models.

Imbens and Rubin (1997b) extend the results of the IV literature (Imbens and Angrist (1994); Imbens and Rubin (1997a); Angrist et al. (1996)) by showing that under the usual assumptions, one can estimate the entire marginal distribution of the outcome under different treatments for the subpopulation of compliers. We briefly explain this method below.[40]

Let $Z_i$ be a binary instrument. Let the pair $D_i(0)$ and $D_i(1)$ denote the values of the treatment for individual i that would be obtained given the instrument $Z_i = 0$ and $Z_i = 1$ respectively. If $D_i(0) = 0$ and $D_i(1) = 1$, unit $i$ is called a *complier*. Let us denote $Y_i(0)$ the outcome that would be observed if the treatment were $D_i = 0$, and $Y_i(1)$ the outcome that would be observed if the treatment were $D_i = 1$.

The population is partitioned by the effect of the treatment assignment on treatment received; for never-takers (units with $D_i(0) = 0, D_i(1) = 0$), let $C_i = n$; for always-takers (units with $D_i(0) = 1, D_i(1) = 1$), let $C_i = a$; finally for compliers (units with $D_i(0) = 0, D_i(1) = 1$); let $C_i = c$. Assuming the monotonicity assumption (the "no defiers" assumption), these three types exhaustively partition the population. Let $\phi_n$, $\phi_a$ and $\phi_c$ denote the populations frequencies of the three types of individuals. These proportions are known to the econometrician.
Although we cannot identify the compliers from the observed data, we can identify some of the non-compliers. If $Z_{obs,i} = 0$ and $D_{obs,i} = 1$, then individual i must be an always-taker with $C_i = a$ and if $Z_{obs,i} = 1$ and $D_{obs,i} = 0$, then individual i must be a never-taker with $C_i = n$. Because of randomisation, the instrument is independent of $C_i$. Hence, in large samples, we know the distribution of $Y_i(1)$ for always-takers (denoted as $g_a(y)$) and the distribution of $Y_i(0)$ for never-takers (denoted as $g_n(y)$).

We are interested in the distributions of $Y_i(0)$ and $Y_i(1)$ among the compliers, denoted as $g_{c0}(y)$ and $g_{c1}(y)$. These distributions cannot be observed directly from the data because among those assigned to $Z_{obs,i} = 0$, both never-takers and compliers will be observed to have

[40] This discussion heavily borrows from Imbens and Rubin (1997b).

$D_{obs,i} = 0$. Analogously, among those assigned to $Z_{obs,i} = 1$, both always-takers and compliers will be observed to have $D_{obs,i} = 1$.

We write the directly estimable distributions of $Y_i$ for the subsample defined by $Z_{obs,i} = z$ and $D_{obs,i} = d$ as $f_{z,d}(y)$. This implies that $g_a(y) = f_{01}(y)$ and $g_n(y) = f_{10}(y)$. Imbens and Rubin (1997b) show that the distributions for the winning and losing compliers can be expressed in terms of the directly estimable distributions in the following way:

$$\begin{aligned} g_{c0}(y) &= \tfrac{\phi_n+\phi_c}{\phi_c} f_{00}(y) - \tfrac{\phi_n}{\phi_c} f_{10}(y) \\ g_{c1}(y) &= \tfrac{\phi_a+\phi_c}{\phi_c} f_{11}(y) - \tfrac{\phi_a}{\phi_c} f_{01}(y) \end{aligned}$$

(2.4)

# Chapter 3

# Does job insecurity deteriorate health?


## Abstract

This paper estimates the causal effect of perceived job insecurity – i.e. the fear of involuntary job loss – on health in a sample of men from 22 European countries. We rely on an original instrumental variable approach based on the idea that workers perceive greater job security in countries where employment is strongly protected by the law, and relatively more so if employed in industries where employment protection legislation is more binding, i.e. in industries with a higher natural rate of dismissals. Using cross-country data from the 2010 European Working Conditions Survey, we show that when the potential endogeneity of job insecurity is not accounted for, the latter appears to deteriorate almost all health outcomes. When tackling the endogeneity issue by estimating an IV model and dealing with potential weak-instrument issues, the health-damaging effect of job insecurity is confirmed for a limited subgroup of health outcomes, namely suffering from headaches or eyestrain and skin problems. As for other health variables, the impact of job insecurity appears to be insignificant at conventional levels.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## 3.1. Introduction

“There’s nothing more deadly than slow growing fear.” Phoebe Killdeer and the Short Straws.

There is evidence in the recent literature that losing one’s job has health-damaging effects[1] which may go as far as inducing a higher risk of mortality.[2] Although job loss is a highly traumatizing event, it is fortunately not very frequent. In contrast, the *fear* of involuntary job loss, i.e. perceived job insecurity, is likely to be much more widespread and one may wonder whether its health impact is as negative as that of actual job loss.

This is an important question from a policy point of view since perceived job insecurity has increased in a large number of industrialised countries over the past twenty years. Following several downsizing episodes in the USA and in Europe, a widely shared view has developed according to which employment relationships have become more unstable than they used to be. Internal labour markets characterised by long careers within firms (Doeringer and Piore (1971)) have been undermined. Long-term employer-employee relationships have declined (Cappelli (1999); Givord and Maurin (2004)) and the labour market seems to have been increasingly working like a spot market (Atkinson (2001)). Correspondingly, the perception of job insecurity has increased in most OECD countries since the 1990s (OECD (2004)).

The importance of job insecurity for workers’ well-being has been underlined in the literature. Böckerman et al. (2011) provide evidence of a strong negative impact of job insecurity on job satisfaction. This impact is actually much stronger than that of the actual type of work contract held by workers – permanent vs. temporary – (Bardasi and Francesconi (2004); Chadi and Hetschko (2013)). In a recent paper, Origo and Pagani (2009) have shown that the level of job satisfaction of workers who do not experience job insecurity[3] is not statistically different whether they have a permanent or a temporary contract. In contrast, workers who feel that their job is insecure are significantly less satisfied than workers who do not, whatever their type of work contract. This suggests that perceived job insecurity is at least as important as the type of work contract in determining workers’ job satisfaction. Since the latter has been shown

[1]See Eliason and Storrie (2009b), Eliason and Storrie (2009a) and Deb et al. (2011).
[2]See Sullivan and Von Wachter (2009) and Browning and Heinesen (2012).
[3]Workers are considered as *not* experiencing job insecurity if they report that it is not very likely or not at all likely that they lose their job in the next 12 months.

to impact individual health (see Fischer and Sousa-Poza (2009a)), perceived job insecurity is also likely to be a key determinant of the health status – potentially even more important than the actual type of work contract.

The literature in epidemiology, occupational psychology and public health has indeed long suggested that job insecurity may be harmful to health because it increases stress (Sverke and Hellgren (2002)). Psychologists have shown that the anticipation of a stressful event represents an equally important or even greater source of anxiety than the event itself (Lazarus and Folkman (1984)). Consistently, job insecurity appears to raise self-reported general and psychological morbidity but also sickness absence and health service use – see the review of the literature by Ferrie (2001). In particular, it is strongly associated with specific symptoms such as eyestrain, skin and ear problems, stomach and sleep disorders (Cheng et al. (2005)). It is also negatively correlated with mental health, as measured by a 30-item psychiatric morbidity scale and a subscale for depressive factors (Ferrie et al. (2005)).

However, evaluating the causal impact of job insecurity on health raises a challenge which requires an adequate identification strategy. Perceived job insecurity is indeed likely to be endogenous. If pessimistic individuals perceive higher job insecurity and, at the same time, report a lower health status, results are likely to be biased. Reverse causality is also likely to be a concern if unhealthy individuals are more likely to be employed in insecure (or, on the contrary, more secure) jobs or if negative health shocks make individuals more likely to fear that they could be fired. In all cases, standard OLS or probit estimates will be biased and will only capture the mere correlation between health and job insecurity.

In this paper, we implement an original identification strategy based on an instrumental variable approach in order to estimate the causal effect of job insecurity on health in a sample of men from 22 European countries. We consider that workers are likely to feel more secure with respect to their job if living in a country where employment is strongly protected by the law, and relatively more so if employed in sectors where employment protection legislation (EPL) is more binding. We thus instrument perceived job insecurity by the stringency of the employment protection legislation in the country where the individual lives interacted with the natural rate of dismissals in the sector where she is employed. This instrument is valid if workers do not self-select into sectors-by-country on the basis of characteristics correlated with their health. We show that this condition holds so that our instrument is truly exogenous. Using cross-country data from the 2010 European Working Conditions Survey (European Foundation for the Improvement of Living and Working Conditions (2012)), we show that when the potential endogeneity of job insecurity is not accounted for, the latter appears to deteriorate almost all health outcomes (self-rated health, suffering from back problems, muscular pain, headaches or eyestrain, stomach ache, depression or anxiety, overall fatigue and insomnia). When tackling the endogeneity issue by estimating an IV model and dealing with potential weak-instrument issues, the health-damaging effect of job insecurity is confirmed for a limited subgroup of health outcomes, namely suffering from headaches or eyestrain and skin problems. As for other health variables, the impact of job insecurity appears to be insignificant at conventional levels.

Our paper contributes to the existing literature in several ways. To our knowledge, we are the first to provide a causal estimate of the impact of perceived job insecurity on health. Most of the literature on this topic estimates mere correlations. Part of it focuses on "attributed" job insecurity as captured by atypical employment (i.e. temporary rather than permanent work contracts) and finds no association between temporary work and general health, but a positive correlation with ill mental health (Bardasi and Francesconi (2004)). The largest strand in this literature deals with perceived job insecurity, as we do. A meta-analysis conducted by Sverke et al. (2002) on 72 papers shows that both physical and mental health are found to decrease as perceived job insecurity increases. However, the magnitude of the effects appears to be ambiguous. On Taiwanese data, Cheng et al. (2005) find that job insecurity is associated with poor self-rated health, with the coefficient being larger for men than for women and, among women, for those employed in managerial and professional occupations. Using a cross-national survey, László et al. (2010) find differences across countries : job insecurity is associated with poor health in the Czech Republic, Denmark, Germany, Hungary, the Netherlands and Poland while the correlation is insignificant in countries such as Austria, France, Greece, Italy, Spain and Switzerland. In all cases, these papers estimate multivariate linear or logistic models disregarding the possibility that job insecurity be endogenous. Mandal et al. (2011) use a different approach : they estimate a random-effect model and use a lagged measure of job insecurity, arguing that this measure is not endogeneous in their data. They find that subjective expectation of job loss is a significant predictor of depression among older workers aged 55 to 65 years old. A few papers take into account the fact that time-invariant omitted variables may

bias their results and estimate fixed-effect models. Using such an approach on Australian data, Green (2011) finds that perceived job insecurity negatively affects mental health. On German data, Reichert and Tauchmann (2012) try to tackle endogeneity issues by instrumenting job insecurity by recent staff reductions in the company where the worker is employed. Thus doing, they show that employees who are concerned about losing their jobs are less psychologically healthy than those in secure jobs. One may wonder, however, whether staff reductions in the company are really uncorrelated with psychological health conditional on job insecurity, which is a necessary condition for their instrument to be exogeneous.
Another attempt to deal with endogeneity issues is made by Ferrie et al. (1995) in a study considering the health impact of in-firm changes potentially incurring job insecurity. The authors use the British Whitehall II sample and exploit the foreseen privatisation of the Property Services Agency, which used to be part of the London-based civil service. More specifically, they use a difference-in-difference approach and compare the health outcomes of those workers who knew they would be affected by privatisation and a control group of civil servants who knew they would not, before and after privatisation was announced. This set-up allows them to estimate the effect of an exogenous shock on firm ownership and organisation on health. The authors find major negative effects on a large range of health outcomes for men, whereas health-damaging effects appear to be milder for women. They interpret these results as providing evidence that job insecurity damages health since expected privatisation must have been associated by civil servants to an increased risk of involuntary job loss. However, Ferrie et al. (1998) show that this very episode of privatisation was associated with major organisational changes. More recent work by Rathelot and Romanello (2012) considers the effect of an episode of major in-firm restructuration in French energy utilities. They find that these restructurations have a strong negative effect on the mental health conditions of the civil servants employed in these companies. As a consequence, using anticipated privatisation as an exogenous shock does not permit to identify the effect of rising job insecurity – as opposed to anticipated organisational changes – on health.

We improve with respect to this literature in two respects. First, using an IV strategy allows us to control for both time-invariant and time-varying omitted variables and/or reverse causality. Second, we are able to identify the causal impact of perceived job insecurity as opposed to any organisational change since our instrument is strongly correlated with the former while it has no reason to vary with firm organisation.

Our research relates in a more indirect way to the literature on job loss and health. Sullivan and Von Wachter (2009) consider the impact of job displacement on mortality in a cohort of Pennsylvanian workers. In order to control for potential selection of displaced workers, they include the mean and variance of individual wages in their estimates as a proxy of productive ability. They show that high-tenure male workers displaced during the early and mid-1980s in the course of mass layoffs experience a 50 to 100% increase in the mortality hazard during the years immediately following job loss. The effect decreases as time passes but converges to a 10-15% increase in the long run. Another strand of literature considers plant closure events in which the whole of the firm's workforce is made redundant. Scholars use propensity score matching (or weighting) methods and compare health outcomes for workers who have been displaced because of closing plant and workers who have stayed in their job in a continuously living plant. On Danish data, Browning et al. (2006) find no evidence of higher risk of hospitalization for stress-related diseases following displacement. Similarly, Eliason and Storrie (2009b) find that displacement does not significantly increase the risk of severe cardiovascular diseases in Sweden. In contrast, they find evidence of a higher probability of hospitalization due to alcohol-related conditions. In a companion paper, they also find higher mortality from alcohol-related conditions and suicides and, to some extent, from ischemic diseases (Eliason and Storrie (2009a)). Similar results for mortality are found by Browning and Heinesen (2012) on Danish data: the risk of mortality is much higher in the displacement group than in the control group. Beyond mortality and hospitalization, Salm (2009) considers a variety of health outcomes and compares those of individuals who lost their job due to plant closure with individuals who did not, before and after the closure of the plant. The results display no significant impact of job loss on health whatever the type of health outcome. Deb et al. (2011) use what they consider a more exogenous measure of job loss than mass layoffs or plant closing, namely business closing. They show that a majority of individuals experience no negative effect of business closing on their BMI and alcohol consumption, while a small minority reports adverse changes. Overall, the literature on job loss has dedicated a lot of effort to properly identify its effect on health outcomes even if the exogeneity of plant or even business closure is still debated - see Deb et al. (2011).

In the present paper, we also try to identify causal effects on health but we focus on perceived job insecurity rather than job loss as the key variable of interest. Both variables are clearly related since job loss may generate job insecurity for survivors or for workers expecting to be fired. However, job insecurity is likely to affect a much larger group of workers since it is a subjective feeling which may not coincide with effective job loss.

The rest of the paper is organised as follows. Section 2 presents our empirical strategy. Section 3 describes the data that we use. Section 4 reports our results and Section 5 concludes.

## 3.2. Empirical Specification

We investigate the impact of perceived job insecurity on health. As a first step, we estimate the following model by a standard probit[4]:

$$Health^*_{ijs} = \alpha + \gamma JobIns_{ijs} + X_{ijs}\beta + D_j + D_s + u_{ijs} \tag{3.1}$$

where $Health^*_{ijs}$ denotes the latent health status of individual $i$ in country $j$ and industry $s$ and is only observed as:

$$Health_{ijs} = \mathbb{1}_{\{Health^*_{ijs}>0\}} \tag{3.2}$$

$JobIns_{ijs}$ denotes the perceived job insecurity of individual $i$ in country $j$ and industry $s$. $X_{ijs}$ is a vector of individual and firm characteristics. $D_j$ and $D_s$ are respectively country and industry dummies and $u_{ijs}$ is an error term.

In some specifications we control for working conditions and psychosocial environment characteristics. The former capture adverse physical working conditions. The latter include indicators of job strain (job pressure, decision latitude and skill discretion) consistent with the Job Demand Control Model proposed by Karasek (1979) as well as a measure of Effort-Reward Imbalance which may be an additional source of job strain according to Siegrist (1996). Both working conditions $WorkCond_{ijs}$ and psychosocial work environment $PsychoSoc_{ijs}$ are indeed likely to be correlated with health and perceived job insecurity. If jobs which are insecure are simply lousy jobs, they may also be characterised by bad working conditions and high job strain. In that case, omitting the latter two variables generates an upward bias in the estimate of $\gamma$. In order to control for both physical working conditions and psychosocial work environment, we estimate the following equation :

$$Health^*_{ijs} = \alpha + \gamma JobIns_{ijs} + X_{ijs}\beta + \mu WorkCond_{ijs} + PsychoSoc_{ijs}\xi + D_j + D_s + v_{ijs} \tag{3.3}$$

[4] All health outcomes are binary variables. Further details are available in the data section.

However, perceived job insecurity $JobIns_{ijs}$ is likely to be endogeneous in which case the probit estimate of $\gamma$ is inconsistent. Endogeneity may arise either from omitted variable bias or reverse causality. As job insecurity and health variables are both self-declared, our estimates are biased if pessimistic individuals systematically tend to report higher job insecurity and lower health status (and the reverse holds for optimistic individuals). Reverse causality is another potential source of bias if unhealthy individuals are more likely to be employed in more insecure (or more secure) jobs. This is also a concern if negative health shocks make individuals fear that they could be fired.

In order to overcome potential endogeneity problems, we jointly estimate the following IV system of 2 equations by conditional maximum likelihood:

$$Health^*_{ijs} = \alpha + \gamma JobIns_{ijs} + X_{ijs}\beta + D_j + D_s + u_{ijs} \tag{3.4}$$

$$JobIns_{ijs} = \delta EPRC_j * DR_{s,USA} + X_{ijs}\zeta + D_j + D_s + \eta_{ijs} \tag{3.5}$$

where $Health^*_{ijs}$ is the latent health status and is only observed as a dichotomous variable (see equation 3.2), $JobIns_{ijs}$ is assumed to be continuous[5], $DR_{s,USA}$ is the dismissal rate in industry $s$ in the USA and $EPRC_j$ denotes the employment protection legislation for regular contracts and collective dismissals in country $j$. Equation (3.4) is the same as (3.1) and equation (3.5) is a linear regression with $EPRC_j * DR_{s,USA}$ as the instrument.

The intuition behind the choice of the instrument is the following. Perceived job insecurity $JobIns_{ijs}$ is likely to be higher in countries where employment protection legislation $EPRC_j$ is less stringent.[6] The index for employment protection legislation is provided by the OECD – see Venn (2009) – and refers to the legislation regarding individual and collective dismissals of workers on regular labour contracts. An additional component of overall employment protection

[5]Our results are robust to dichotomising job insecurity – by opposing those who either disagree or strongly disagreee with the idea that they may lose their job in the next six months and those who neither agree nor disagree, agree and strongly agree with this statement – and running a 2SLS estimation of equation (3.4) where dichotomised job insecurity is instrumented by $EPRC_j * DR_{s,USA}$.

[6]In contrast, Clark and Postel-Vinay (2009) suggest that employment protection legislation is negatively correlated with the *satisfaction* with job security. According to them this negative correlation is due to the fact that their *satisfaction* variable captures two components of job security : the probability of job loss and the cost of it. The former decreases with EPL – which is consistent with our assumption – but the latter strongly rises with EPL since finding a new job is quite harder in countries where employment is strongly regulated.

legislation has to do with regulations of temporary work contracts. We do not include it in our EPL index (and restrict our sample accordingly to permanent workers) because it is not clear whether the rules restricting the use of temporary contracts actually protect temporary workers or rather permanent ones, by making temporary work either more costly or less convenient to use (OECD (2014)).

Of course, the stringency of employment protection legislation cannot be used, *per se*, as an instrument since its variability would be very low and it would capture all heterogeneity existing across countries. This is why we instrument job insecurity by the stringency of employment protection legislation $EPRC_j$ in the country where the individual lives interacted with the extent to which EPL is binding in the sector where the individual is employed. As is classical in the job and worker flow literature – see Bassanini et al. (2009) and Haltiwanger et al. (2014) – we consider that EPL is particularly binding in sectors where the natural rate of dismissal is high. We proxy the latter by the industry-level dismissal rate in the USA. The reason for choosing this country as a benchmark is that EPL is almost nonexistent in the USA – see Venn (2009) – so that the observed dismissal rates may be considered as capturing the natural dismissal propensity in the corresponding industries.

Overall, the assumption underlying our instrument is that workers living in countries with a strong employment protection legislation will feel comparatively more secure, as far as their job is concerned, when employed in industries with a high natural rate of dismissal because this is where the stringency of EPL makes more difference. Our instrument is valid if workers do not self-select into sectors-by-country on the basis of characteristics which may be correlated with their health. We will provide evidence that this is not the case in Section 3.3..

Note that our instrument captures the risk of being dismissed which is likely to be a good predictor of the perceived risk of losing one's job, i.e. our job insecurity indicator. Finding a good instrument would have been more complicated should our variable of interest have been the individual's satisfaction with her job security. The latter is indeed likely to be determined not only by the risk of losing one's job, but also by the expected level of unemployment benefit and the probability of re-employment if dismissed. In the present case, our job insecurity variable captures the perceived risk of dismissal which is easier to predict since it does not depend on expectations about future well-being but only on the actual risk of dismissal faced by the individual.

## 3.3. Data

### 3.1. Presentation of the sample

We use the fifth wave of the European Working Conditions Survey (EWCS). Since its launch in 1990, the EWCS measures and monitors trends and changes in working conditions in Europe. It has been conducted every five years on a random sample of workers (salaried employees and self-employed) in a growing number of European countries (from 12 in 1990 to 34 in 2010).

The European Foundation for the Improvement of Living and Working Conditions commissioned the fifth wave of the EWCS to be carried out in winter-spring 2010. Face-to-face interviews were conducted with persons in employment in the 28 member states as well as in Norway, Macedonia, Turkey, Albania, Kosovo and Montenegro. The questionnaire covers issues such as employment status and the general job context : working time, work organisation, earnings and financial security, job insecurity, psychosocial work environment, work-life imbalance and access to training. It also covers several aspects of health, well-being and psychological conditions as well as demographic and socio-economic characteristics. Importantly, the 2010 EWCS questionnaire includes a detailed health module. Previously, the EWCS had only a few questions related to health, such as “Do you think your health or safety is at risk because of your work?” or “Does your work affect your health, or not?”. The formulation of these questions was problematic, as it was likely to suffer from framing effects. This is why we do not use EWCS waves prior to the 2010 one. Response rates in the 2010 wave vary substantially across countries from 31.3% in Spain to 73.5% in Latvia with an average response rate of 44.2% across all countries – see the Fifth EWCS Technical Report (2010). As underlined in the technical report, “EWCS had lower-than-desired response rates particularly in countries reporting low response rates in similar random face-to-face social surveys : Poland, Slovenia, the United-Kingdom, France, Belgium and the Netherlands”. In the 2010 wave almost 44,000 workers were interviewed. The original sample included all persons aged 15 and above who were resident in the country that was being surveyed and who were in employment[7] during the reference week.

Our empirical strategy uses the employment protection legislation index for individual and collective dismissals of workers on regular work contracts (EPRC). This index is available for

[7] Being in employment was defined as having done any work for pay or profit during the reference week for at least one hour.

only 22 countries (out of 34).[8] Moreover, as it is defined only for individuals employed with a regular contract in the business sector, we exclude from the sample self-employed individuals, individuals working in non-business sectors[9], as well as individuals who did not have a regular work contract at the time of the survey. As is standard in the literature – see OECD (2010b) – we also exclude individuals working very short hours (less than 15 hours during the reference week). We further restrict our sample to men only since in our data women are overrepresented in very small establishments (less than 5 employees)[10] for which the scope of employment protection legislation is reduced in most countries. Overall, our final sample consists of 5,541 men across 22 countries. Once conditioning on having no missing value on any dependent variable and/or covariate, our sample goes down to 4,749 observations for all health outcomes. Table A-3.1 gives a overview of the sample restrictions we make and displays the number of observations dropped after each sample restriction.

### 3.2. Variables

Perceived job insecurity is assessed by asking workers their opinion about the following statement : "I might lose my job in the next 6 months". Five answers are available ranging from "strongly agree" to "strongly disagree".[11] We standardise job insecurity to mean 0 and 1 standard deviation.

Measuring health using survey data is always a challenge. The EWCS questionnaire includes a question on self-rated health where respondents are asked to rate their health on a 5-point scale : very good, good, fair, bad or very bad. We dichotomise the responses into good (very good and good) and bad health (fair, bad or very bad). There is evidence in the literature that self-rated health is a good indicator of individual overall health (Ferrie et al. (1995)). It has been found to be a good predictor of mortality even after controlling for more objective measures of health (Idler and Kasl (1991); Idler and Benyamini (1997); Bath (2003)). However, the probability of reporting good or bad health may suffer from individual reporting heterogeneity (Etilé and Milcent (2006); Tubeuf et al. (2008)). This is why we also use more objective measures

[8]The EPRC index is available for the following countries : Austria, Belgium, the Czech Republic, Germany, Denmark, Spain, Finland, France, the United-Kingdom, Greece, Hungary, Ireland, Italy, the Netherlands, Norway, Poland, Portugal, the Slovak Republic, Sweden, Turkey, Slovenia and Estonia.

[9]Agriculture, mining and fuel are excluded too because of problems of data reliability, so that the sectors included in our study correspond to sectors 15 to 74 in the NACE Rev. 1 classification.

[10]They have a 60% higher probability than men to be employed in very small establishments.

[11]This is a standard way to measure perceived job insecurity in the literature. For example, in the Karasek's Job Content Questionnaire (JCQ), job insecurity is measured on a 4-point scale by the proposition "My job is secure", where response categories range from "strongly agree" to "strongly disagree" (Karasek et al. (1998)).

of health capturing specific diseases or symptoms. In the EWCS database, respondents are asked whether they have suffered over the last 12 months from either backache, skin problems, muscular pain in shoulders, neck and/or upper limbs, muscular pain in lower limbs, headache or eyestrain, stomach ache, cardiovascular diseases, depression or anxiety, overall fatigue, or insomnia or general sleep difficulties. For each above-mentioned health disorder, we build a corresponding dummy variable taking value 1 if the individual suffered from it, 0 otherwise.
We also use some information on individuals' well-being. We build a dummy variable equal to 1 if the individual answers "All the time", "Most of the time" or "More than half of the time" to at least one of the following assertions : "[Over the past two weeks] I have felt cheerful and in good spirits"; "I have felt calm and relaxed"; "I woke up feeling fresh and rested"; "My daily life has been filled with things that interest me". Our well-being dummy indicator is equal to 0 otherwise.

Our baseline specification includes a set of covariates capturing individual and firm characteristics. Some specifications also control for working conditions and psychosocial work environment.
Individual and firm characteristics include age (entered as a continuous variable), the presence of a spouse or partner in the household, occupation[12] (managers and professionals, technicians and supervisors, white collars, blue collars) and education[13] (higher education, secondary education, below secondary). As the income variable in the EWCS has many missing values and is not quite reliable, we use a question on the "household's ability to make ends meet given its total monthly income". We build a dummy variable equal to 1 if individuals report that their household makes ends meet "with some difficulty", "with difficulty" or "with great difficulty", and equal to 0 otherwise. We interpret this indicator as a measure of households' deprivation. We also use a question reporting whether the individual was unemployed immediately before this job (dummy variable equal to 1 if so, 0 otherwise), information on establishment size (five classes) and the presence of an employee representative at the workplace (dummy variable equal to 1 if so, 0 otherwise).
Working conditions are captured by an index taking values 0 to 10, where 10 denotes adverse working conditions. It is the normalised sum of 15 dummy variables taking value 1 if the individual is exposed half of the time or more to a given working condition, and 0 otherwise. The 15

[12]Based on the 1988 International Standard Classification of Occupations (ISCO 88).
[13]Based on the International Standard Classification of Education (ISCED).

working-condition components are : being exposed to vibrations from hard tools or machinery; to noise so loud that one would have to raise one's voice to talk to people; high temperatures which make one perspire even when not working; low temperatures whether indoors or outdoors; breathing in smoke, fumes, powder or dust; in vapors such as solvents and thinners; handling or being in skin contact with chemical products or substances; breathing tobacco smoke from other people; handling or being in direct contact with materials which can be infectious, such as waste, bodily fluids, laboratory materials; having a job that involves tiring or painful positions; lifting or moving people; carrying or moving heavy loads; standing; performing repetitive hand or arm movements; handling angry clients or patients.

As for psychosocial work environment characteristics, they are measured through a series of indicators adapted from the Job Content Instrument of Karasek (Karasek (1979)) and the Effort-Reward Imbalance Questionnaire (Siegrist (1996)). These indicators include job pressure, decision latitude, skill discretion and reward, and are measured as follows. Job pressure is built out of three components : not having enough time to get the job done (measured on a 5-point scale where response categories range from "always" to "never"), working at high speed (7-point scale ranging from "all the time" to "never"), and working to tight deadlines (7-point scale ranging from "all the time" to "never"). We combine the responses into a summary scale and normalise it to [0;10], where 10 denotes high job pressure. We then divide the scale into tertiles, i.e. low job pressure, moderate job pressure and high job pressure. A measure of decision latitude is obtained using three dummy variables : the ability to choose or change the order of tasks, the methods of work and the speed or rate of work (all variables taking value 1 if the individual has control over the corresponding decision, 0 otherwise). We combine the responses into a summary scale, normalise it to [0;10], where 10 denotes high decision latitude, and divide it into tertiles. Skill discretion is measured by a single question asking whether one's job involves learning new things (dummy variable equal to 1 if so, 0 otherwise). Finally, workers' reward is assessed by two questions : being well paid to do one's work (measured on a 5-point scale where response categories range from "strongly disagree" to "strongly agree"); having a job that offers good prospects for career advancements (5-point scale ranging from "strongly disagree" to "strongly agree"). Responses are summed into a summary scale that is normalised to [0;10] and divided into tertiles.

## 3.3. Instrument

We instrument perceived job insecurity by the stringency of employment protection legislation EPRC in the country where the worker lives interacted with the US rate of dismissals in the industry where he is employed. We borrow US dismissal rates from Bassanini and Garnero (2013). Their database contains dismissal rates over 1996-2006 and uses an industry classification that can be matched, at a sufficiently disaggregated level, to the Nace Rev. 1 classification used in the EWCS. To capture the natural dismissal propensity at the industry level, we compute a quantitative indicator equal to the average US industry dismissal rate between 2000 and 2006.[14] Overall, we have information on 23 industry-level US dismissal rates.

Data on employment protection legislation are provided by the OECD. The EPRC index that we use refers to the legislation regarding individual and collective dismissals of workers on regular labour contracts and varies at the country level. As regards individual dismissals, it is built out of information on notification procedures, delays before the notice period can start, the length of the notice period and size of severance payments, the circumstances under which a dismissal is considered unfair and compensation and extent of reinstatement following unfair dismissal. Regarding collective dismissals, the index takes into account the number of workers above which dismissals are considered as collective as well as additional notification and delay requirements and other special costs to employers.[15] The theoretical value of the EPRC index varies from 0 to 6 (where 6 is the most stringent legislation).[16] The list of industries and countries that we use, together with the US sectoral dismissal rates and the national EPRC indices can be found in Appendix Table A-3.5.

## 3.4. Descriptive statistics

Figure 3.1 and Tables A-3.2, A-3.3 and A-3.4 provide the descriptive statistics for our sample. As shown in Figure 3.1, 32% of the workers strongly disagree with the statement that they might lose their job in the next six months, while 34% simply disagree, 18% neither agree nor

[14] Following the evidence provided by Bassanini et al. (2009), we assume that the natural dismissal propensity in the USA is stable over time and we average it over a complete cycle, 2000-2006.

[15] Further details on the construction of the employment protection index can be found in Venn (2009).

[16] The EPRC index that we use refers to year 2008. We pre-date it because, over the period under study, a number of EU countries implemented reforms of employment protection legislation. Given that it takes a while for employees to understand how the new rules really work, people tend to base their expectations on prior information.

disagree, 12% agree and 4% strongly agree. In the sample, the average age is 41 years old, 71% of our individuals live with a spouse or partner, and 35% report having difficulties to make ends meet. 7% report having had a period of unemployment immediately before their current job, and 48% have an employee representative at their workplace. A majority of workers in our sample (61%) are employed in establishments with less than 50 employees, while only 9% are employed in large establishments (more than 500 employees). While 78% of individuals declare being in good health (good or very good self-rated health), we do see a number of health disorders – see Table A-3.3. 47% of workers report suffering from backache, 43% from muscular pain in upper limbs, 30% from muscular pain in lower limbs, 34% from headache or eyestrain, 34% from overall fatigue and 18% from insomnia or sleep difficulties. However, fewer workers report suffering from skin problems (8%), stomach ache (12%), cardiovascular diseases (5%), or depression or anxiety (8%). 93% of the individuals in the sample experienced well-being the week preceding the interview. We also control for the industry where the worker is employed. The largest proportions of respondents are found in the construction sector (15%), in renting and business activities (10%) and in retail trade (10%) – see Table A-3.4. We also provide a country-by-country breakdown of our sample. Belgium, France and Germany are the most represented countries and Ireland is the country with fewest respondents.

## 3.4. Results

### 3.1. Probit estimates

Probit estimates of equations (3.1) and (3.3) are reported in Table 3.1. Each line presents the point estimate (resp. standard error) of perceived job insecurity ($\hat{\gamma}$) for a different health outcome.[17] In column 1 we only control for individual and firm characteristics, i.e. age, education, occupation, marital status, difficulties to make ends meet, period of unemployment immediatly before current job, establishment size, presence of an employee representative in the establishment where the person is employed, industry and country dummies. Job insecurity appears to

[17] The point estimates and standard errors on individual and firm controls are reported in Appendix Table A-3.6 for one particular health outcome, namely self-rated health. As could be expected, age is negatively correlated with self-rated health. When controlling for education, occupation does not appear to be significantly correlated with health. Living with a spouse or partner, establishment size and the presence of employee representatives in the establishment do not seem to significantly affect self-rated health either. In contrast, having problems to make ends meet is associated with poorer self-rated health which is unsurprising if this variable captures to some extent low income levels. Suprisingly enough, being unemployed immediately before the current job is associated with better health (at the 10% significance level).

be positively correlated with all health disorders in our data except skin problems and cardiovascular diseases. In particular, it is associated with a long series of physical troubles (back problems, muscular pain, headaches or eyestrain, stomach ache) as well as with depression or anxiety, overall fatigue and insomnia, all of these at the 1% significance level. When computing average marginal effects[18], we find that the impact of a one-standard-deviation increase in job insecurity on the probability of reporting health disorders ranges from 1.9% for stomach ache to 4.2% for muscular pain in upper limbs. Unsurprisingly, job insecurity is also associated with poorer self-rated health. Coefficients in Table 3.1 imply that when job insecurity increases by 1 standard deviation, the probability of reporting bad self-rated health increases by 3% on average in our sample. Beyond its health-damaging effect, we also find that job insecurity decreases the probability of reporting at least one dimension of well-being over the past two weeks (either feeling cheerful or relaxed or rested or having an interesting life). So, job insecurity appears to be uniformly harmful to health and to our measure of well-being.

Results are very similar when controlling for bad physical working conditions – see column (2). Whatever the health outcome or well-being variable we consider, the point estimate on job insecurity is slightly lower than when we do not include any indicator of working conditions. However, its magnitude remains in the same range as in column (1) and it is highly significant at conventional levels, except for skin problems and cardiovascular diseases. The same pattern of results is also found when adding psychosocial factors to our specification – see column (3). A one-standard-deviation increase in job insecurity increases the probability of reporting bad self-rated health by 1.9%.[19]

Overall, the results from these simple probit estimates are consistent with most findings in the literature suggesting that job insecurity is associated with ill physical and mental health and with lower well-being (Ferrie (2001)).

[18] Average marginal effects are computed by first calculating the marginal effect for each observation and then averaging over the entire sample.

[19] The point estimates and standard errors on working conditions and psychosocial work environment characteristics are reported in Appendix Table A-3.6 for one specific health outcome – i.e. self-rated health. Unsurprisingly, bad working conditions deteriorate self-rated health. Low job pressure is associated with better health than high job pressure. As suggested by Siegrist (1996), higher rewards for given effort levels are important to workers' well-being and they appear to be correlated with better self-rated health. The same holds for high decision latitude which appears to be positively correlated with self-reported health.

## 3.2. IV estimates

### 3.2..1 Baseline estimates

However, as mentioned in section 3.2., job insecurity is likely to be endogenous both because of potential omitted variable bias and of reverse causality. In order to deal with this issue, we estimate an instrumental variable probit in which $JobIns_{ijs}$ is instrumented by the stringency of employment protection legislation in the country where worker $i$ lives interacted with the natural rate of dismissals in the industry where he is employed. Results obtained when estimating equation (3.5) are reported in Table 3.2.[20] As expected, we find that workers living in countries with more strigent EPL feel comparatively less insecure when employed in sectors characterised by a high natural rate of dismissals. When controlling for both bad working conditions and psychosocial work environment, the estimates yield very similar results.

When instrumenting job insecurity, our estimates[21] of equation (3.4) suggest that it does damage a limited number of health outcomes – see Table 3.3. Results in column (1) show that job insecurity increases the probability of reporting poor self-rated health and this effect is significant at the 5% level. It also raises the frequency of a couple of more specific health symptoms, namely skin problems and headaches and/or eyestrain – with both point estimates significant at the 1% level.[22] Surprisingly, overall fatigue seems to decrease with job insecurity, although the effect is not highly significant in all specifications. As regards the other health outcomes, the coefficients of job insecurity are not statistically significant. As evidenced in columns (2) and (3), these findings are robust to controlling for working conditions and/or psychosocial work environment : the point estimates remain stable across specifications.[23]

[20]Equation (3.5) is jointly estimated with equation (3.4). The estimates shown in Table 3.2 are obtained when the health outcome on the left-hand side of equation (3.4) is self-rated health. The coefficients and standard errors on all control variables are reported in Appendix Table A-3.7. All standard errors are clustered at the country*industry level (466 clusters).

[21]All standard errors are clustered at the country*industry level.

[22]These results are robust to removing one country at a time from our sample. When doing so, the point estimates remain in the same order of magnitude – ranging from 0.719 to 0.926 for skin problems and from 0.629 to 0.871 for headaches and/or eyestrain and significant at the 1% level. The same holds for self-assessed health with coefficients ranging from -0.606 to -0.911 – significant at the 5% level – except when removing Slovenia, Denmark or Finland in which case the point estimates get lower (around -0.500) and are no longer significant at conventional levels. The fact that our results are not quite as robust for self-assessed health as for skin problems and headaches/eyestrain will be confirmed lower down in this section when estimating weak-instrument-robust confidence intervals.

[23]This suggests that the IV exclusion restriction is likely to hold without conditioning on working conditions and psychosocial work environment.

One concern with these results is that the point estimates reported in Table 3.3 are much larger than those estimated by naive probit[24] and the corresponding standard errors are also quite large.[25] This increase in the coefficients when estimating the IV model may, of course, be due to the combined outcome of many potential sources of endogeneity. Measurement error may be one of those (Card (2001)). Another source of endogeneity may also arise from unhealthy individuals self-selecting into more secure jobs, in which case the naive probit coefficients would underestimate the true health effect of job insecurity. It should also be noted that each IV estimate can be given a causal interpretation as a Local Average Treatment Effect (LATE) without requiring constant treatment assumption. In our case, the "treatment" is defined as being job insecure, and the IV estimate is identified on the subset of individuals whose behaviour is shifted by the instrument, i.e, the compliers. The observed increase in the IV coefficients may be explained by the compliers' specific characteristics. More specifically, the compliers in our setup are individuals whose perceived job insecurity is determined by the interaction between EPL and the natural rate of dismissals in the sector where they are employed. Although it is not an easy task to characterize them, we hypothesize that the interaction between EPL and the industry-specific layoff rate is probably more binding for individuals whose subjective expectations depend on the law. If job insecurity is particularly harmful to health for this specific subset of individuals, this may explain the increase in our IV estimates.[26]

However, one could also worry that our large IV estimates be due to a weak instrument problem since the F-test of the excluded instrument in equation (3.5) is slightly below 10.[27] To tackle this issue, we derive weak-instrument-robust confidence intervals for the impact of job insecurity on each of our health outcomes. In doing this, we follow the method proposed by

[24] This increase in the coefficients does not seem to be due to the estimation method that we use : when estimating our model by 2SLS the coefficients we obtain are in the same range of magnitude as the average marginal effects corresponding to the point estimates presented in Table 3.3. Results are available upon request.

[25] Note that, using our complete specification, the coefficients estimated for self-rated health, skin problems and headaches/eyestrain are significantly different from those estimated by probit since the confidence intervals do not overlap. For self-rated health, the confidence interval of the IV estimate is [-1.227;-0.252] whereas it is [-0.122;-0.031] for the probit estimate. For skin problems the corresponding intervals are [0.512;1.213] and [-0.040;0.079]. For headache/eyestrain, they are [0.449;1.138] and [0.035;0.115]. In contrast, for overall fatigue, the IV and probit estimates are not statistically different.

[26] Another way to look at this issue is to determine which sectors or countries are the most shifted by the instrument. To investigate this, we compute the correlation between observed and predicted job insecurity both at the country and industry level. Note that for simplicity's sake, we predict job insecurity using the first stage of a 2SLS model. We find that observed job insecurity is particularly well-predicted in countries such as Belgium, Germany, Denmark and Greece; and in industries such as renting and business activities, transport and storage as well as in the construction sector.

[27] For all health outcomes, the F-test of the excluded instrument is about 9 in the baseline specification, 9 when controlling for working conditions and 11 when controlling both for working conditions and psychosocial factors.

Boeri et al. (2012) who extend to non-linear models the reduced-form approach developed by Angrist and Krueger (2001) and Chernozhukov and Hansen (2008) for linear models.
More specifically, as suggested by Boeri et al. (2012), we first define $A$ as a wide enough range of potential values for $\gamma$ in equation (3.4). For each $a \in A$, we rewrite equation (3.4) as follows :

$$Health^*_{ijs} = \alpha + (\gamma - a)JobIns_{ijs} + aJobIns_{ijs} + X_{ijs}\beta + D_j + D_s + u_{ijs} \tag{3.6}$$

We then replace the first instance of $JobIns_{ijs}$ by its expression in equation (3.5) :

$$\begin{aligned} Health^*_{ijs} = \alpha + \delta(\gamma - a)EPRC_j * DR_{s,USA} + aJobIns_{ijs} + X_{ijs}[\zeta(\gamma - a) + \beta] \\ +D_j + D_s + (\gamma - a)\eta_{ijs} + u_{ijs} \end{aligned} \tag{3.7}$$

We then estimate equation (3.7) as a constrained probit, forcing the coefficient of the endogenous variable $JobIns_{ijs}$ to equal $a$. By doing so, the endogeneity of $JobIns_{ijs}$ becomes irrelevant for the consistent estimation of $\delta(\gamma - a)$. In such a modified reduced-form equation, the usual test statistics for the significance of $\delta(\gamma - a)$ tests the null $\gamma = a$ (conditional on $\delta \neq 0$). Iterating over several values of $a$ allows constructing a confidence interval for $\gamma$ that is robust to weak instruments since it does not use information about the strength of the correlation between the instrument and the endogenous variable.
In practice, we proceed as follows :
1. We set $A$ as the set of real numbers in $[m_1; m_2]$[28], spaced 0.01.
2. We estimate equation (3.7) for each $a \in A$ and retain the z-statistics for $\delta(\gamma - a)$.[29]
3. We construct the $1-p$ confidence interval as the set of $a$'s such that the z-statistics is smaller than $c(1 - p)$ where $c(1 - p)$ is the $(1 - p)^{th}$ percentile of a $\chi^2_1$ distribution.

Applying this procedure yields a 95% confidence interval for $\gamma$. For headaches or eyestrain, this interval is [0.37;2.46] which has to be compared to the narrower interval [0.45;1.14] derived from the usual maximum likelihood asymptotics. As regards skin problems the corresponding intervals are [0.1;2.66] and [0.51;1.21]. What matters here is that, for both health outcomes, the intervals only contain strictly positive values, which confirms that the positive impact of job

[28]For each health outcome, we choose $[m_1; m_2]$ so that it contains a wide enough range of potential values for $\gamma$. For headaches and/or eyestrain it is set, for example, to [-1;2.5].
[29]Note that under the null, the term $(\gamma - a)\eta_{ijs}$ disappears from equation (3.7), thus simplifying its estimation.

insecurity on headache and/or eyestrain and skin problems that we estimate is robust to potentially weak instruments. In contrast, for all other health outcomes – including self-rated health and overall fatigue – the weak-instrument-robust confidence intervals systematically contain 0 so that the impact of job insecurity is not significant at conventional levels when estimated in this conservative way.

Overall, this method allows us to derive weak-instrument-robust confidence intervals from reduced-form estimates. The price to pay for this is that we cannot derive precise point estimates for the impact of job insecurity on health outcomes since the corresponding confidence intervals are very large. In contrast, it allows us to claim with a high degree of confidence that job insecurity has a positive causal impact on the probability of reporting headaches and/or eyestrain and skin problems.

## 3.3. Robustness checks

One may worry that unhealthy workers might self-select into low-dismissal industries and that this selection pattern may vary according to country-specific levels of EPL. If this were the case, our instrument would no longer be valid since the identifying assumption – according to which workers do not self-select into sectors-by-country on the basis of a characteristic correlated with health – would not hold anymore. In order to test for this, we estimate the following equation :

$$HighDismiss_{ijs} = \lambda + \xi Health_{ijs} + \psi Health_{ijs} * EPRC_j + X_{ijs}\theta + D_j + \upsilon_{ijs} \tag{3.8}$$

where $HighDismiss_{ijs}$ is a dummy variable equal to 1 if worker $i$ is employed in a high-dismissal industry and 0 otherwise. Other variables are defined as in Section 3.2.. We use different definitions of high-dismissal industries : industries with dismissal rates higher than (i) the median, (ii) the third quartile and (iii) the upper decile. Whatever the threshold we use for defining high-dismissal industries and whether or not we control for job insecurity in the regression, $\hat{\psi}$ is never significant at conventional levels.[30] In an alternative specification,

[30] When high-dismissal industries are defined as industries with dismissal rates higher than the median, the point estimate of $\hat{\psi}$ is -0.071 – with standard error 0.061 – when controlling for firm and individual characteristics along with working conditions and psychosocial factors. When adding job insecurity as an additional control, the point estimate of $\hat{\psi}$ is -0.074 with standard error 0.061.

we estimate a multinomial probit where the outcome variable is the sector in which the worker is employed.[31] The point estimates associated with the interaction term $Health_{ijs} * EPRC_j$ are never significant. This suggests that workers do not self-select into industries on the basis of their health status in a different way according to the level of EPL in their home country. Hence, our IV is valid to uncover the causal impact of job insecurity on health.

Another concern has to do with potential sample selection bias. If high-dismissal industries tend to rely more on temporary contracts in high-EPL countries in order to meet their needs in terms of labour force turnover, a disproportionate part of their workforce will be left out of our sample to the extent that we exclude temporary workers. If unhealthy workers are more likely to be employed on temporary contracts than healthy ones, workers employed in high-dismissal/high-EPL sectors*countries in our sample are likely to enjoy a better health status than those employed in high-dismissal/low-EPL sectors*countries. To the extent that our instrument predicts a lower job insecurity for workers employed in high-dismissal/high-EPL sectors*countries, we may overestimate the negative health impact of job insecurity. We check that the probability of being employed on a temporary contract is not higher in high-dismissal/high-EPL sectors*countries than in high-dismissal/low-EPL sectors*countries. On the sample of permanent and temporary workers, we regress the probability of holding a temporary contract on the $EPRC_j * DR_{s,USA}$ interaction.[32] The coefficient on the interaction term is insignificant with a point estimate of 0.058 (standard error : 0.070), which suggests that our results are unlikely to be driven by selection bias due to the exclusion of temporary workers.

Our results derive from estimates run on a sample of workers aged 15 years old and above. However, senior workers may overreact to job insecurity since in most countries, their probability to get back to employment if dismissed is lower than for younger workers (OECD (2011)). In this case, one could be afraid that our results be driven by a particularly strong effect of job insecurity on health for this specific age group. We check that our findings are robust to the exclusion of older workers by re-running our complete IV estimates[33] on the group of prime-age

[31]Sectors are aggregated as follows : "Manufacturing, electricity, gas and water supply", "Construction", "Wholesale and retail trade; repair of motor vehicles, motorcycles and personal and household goods", "Hotels and restaurants", "Transport, storage and communication" and "Financial intermediation, real estate, renting and business activities".

[32]This specification includes controls for individual and firm characteristics together with working conditions and psychosocial factors, as well as country and industry dummies.

[33]This specification includes controls for individual and firm characteristics together with working conditions and psychosocial factors, as well as country and industry dummies.

workers (aged 25 to 59). The results are virtually unchanged.[34] Unfortunately, we cannot run similar estimates on the younger and older age groups since the number of observations is too low (294 and 215 respectively) to allow us to properly estimate our model.

Controlling for a measure of income when explaining individual health differences is standard in the literature (Lundborg (2013)). Given the scarce quality of income data in the European Working Conditions Survey, we use information on "problems to make ends meet" as an alternative in our baseline specification. However, one could be concerned that this variable might be endogenous if unhealthy workers have got problems making a living. In order to make sure that this does not generate a bias in our estimates, we re-estimate our complete IV specification dropping this covariate. The results are essentially unaffected.[35]

## 3.5. Conclusion

In this paper, we provide evidence of the causal effect of perceived job insecurity on various health outcomes in a sample of men from 22 European countries. We instrument perceived job insecurity by the stringency of employment protection legislation in the country where the individual lives interacted with the natural rate of dismissals in the industry where he is employed. Using cross-country data from the 2010 European Working Conditions Survey, we show that when the potential endogeneity of job insecurity is not accounted for, the latter appears to deteriorate almost all health outcomes (self-rated health, suffering from back problems, muscular pain, headaches or eyestrain, stomach ache, depression or anxiety, overall fatigue and insomnia). When tackling the endogeneity issue by estimating an IV model and deriving weak-instrument-robust confidence intervals, findings are more mixed. The health-damaging effect of job insecurity is confirmed for a limited subgroup of health outcomes, namely the probability of suffering from headaches or eyestrain and skin problems. In contrast, the impact of job insecurity on other health variables comes out as insignificant. Our results are robust to controlling for individual and firm characteristics but also for adverse working conditions and psychosocial environment characteristics.

[34] The point estimates (resp. standard errors) are -0.869 (0.173) for self-rated health, 0.839 (0.219) for skin problems and 0.800 (0.190) for headaches and eyestrain.

[35] The point estimates (resp. standard errors) of the job insecurity variable are -0.735 (0.250) for self-rated health, 0.855 (0.179) for skin problems and 0.788 (0.176) for headaches and eyestrain.

The method that we use does not allow us to derive precise point estimates. However, we show that the fear of involuntary job loss has clear worsening effects on two specific health disorders, i.e. headaches and/or eyestrain and skin problems. As regards other health outcomes, job insecurity does not seem to have any significant impact. This could suggest that feeling insecure with respect to one's job is not quite as bad for health as losing it. However, let us underline that the method that we use to derive weak-instrument-robust confidence intervals is extremely conservative, so that our results cannot be interpreted as ruling out any damaging impact of job insecurity on those outcomes. Moreover, we only capture short-term effects here, so that we cannot exclude that job insecurity might have a more negative impact in the longer-run if its health-damaging effects cumulate over the years.

Overall, our findings confirm the results obtained in epidemiology and occupational health, i.e. that low job security is related to somatic morbidity (Mohren et al., 2003; Ferrie et al., 2002). Karasek (1979) suggested long ago that "work's psychological burden consists not only of the work of carrying out the task but also in the human costs of adapting to labor market dynamics". Our results are evidence of that.

The health-damaging effects that we find for a couple of health outcomes raise the issue of the mechanisms through which perceived job insecurity affects both mental and physical health. The psychology literature has long emphasised the role of stress. Another (complementary) explanation might be that workers who are afraid of losing their job tend to increase precautionary savings and hence reduce investments, in particular in health. The lack of information about health consumption in our data does not allow us to test such a hypothesis. Moreover, it is unclear how relevant this mechanism may be since time is one of the most important inputs in health investments. In any case, investigating the consequences of job insecurity for health investments would be extremely valuable and improve our understanding of the mechanisms through which the fear of job loss deteriorates health.

Whatever the mechanism through which perceived job insecurity affects health, this effect is likely to be stronger for workers with low employability, i.e. with a low probability of finding a new job if losing the current one. According to Green (2011) employability is indeed a key determinant of the impact of job insecurity upon job satisfaction. Unfortunately, the information available in the EWCS database does not allow us to tackle this issue properly. A promising avenue for future research would consist in investigating the potential role of employability on the health-damaging effects of perceived job insecurity using reliable measures of employability.

# Tables and Figures

**Figure 3.1** – Descriptive statistics : Job insecurity distribution.

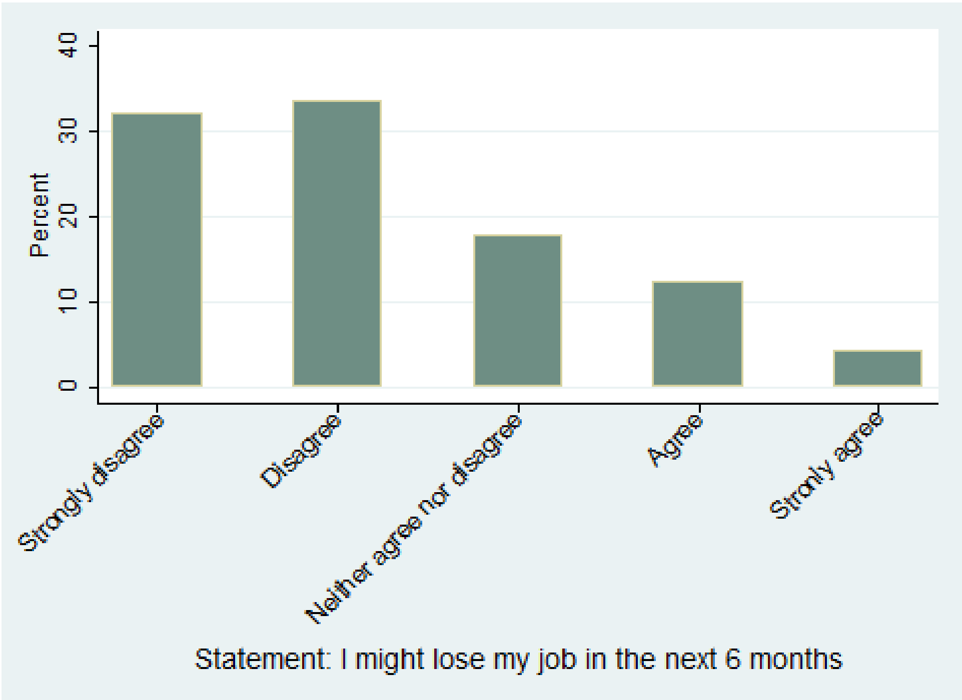

**Table 3.1** – Probit model : Coefficients of job insecurity

| Health outcome | Baseline<br><br><br>(1) | Baseline<br>+Working conditions<br><br>(2) | Baseline<br>+Working conditions<br>+Psychosocial factors<br>(3) |
|---|---|---|---|
| Self-rated health | -.116*** | -.110*** | -.077*** |
| | (.023) | (.023) | (.023) |
| Backache | .095*** | .084*** | .068*** |
| | (.020) | (.020) | (.021) |
| Skin problems | .042 | .033 | .019 |
| | (.029) | (.029) | (.030) |
| Muscular pain in upper limbs | .114*** | .105*** | .084*** |
| | (.020) | (.020) | (.021) |
| Muscular pain in lower limbs | .073*** | .061*** | .047** |
| | (.021) | (.021) | (.022) |
| Headaches, eyestrain | .096*** | .091*** | .075*** |
| | (.020) | (.020) | (.020) |
| Stomach ache | .098*** | .096*** | .081*** |
| | (.025) | (.025) | (.025) |
| Cardiovascular diseases | -.009 | -.014 | -.026 |
| | (.039) | (.039) | (.040) |
| Depression, anxiety | .181*** | .173*** | .147*** |
| | (.029) | (.029) | (.029) |
| Overall fatigue | .095*** | .087*** | .062*** |
| | (.021) | (.021) | (.021) |
| Insomnia, sleep difficulties | .133*** | .127*** | .104*** |
| | (.023) | (.023) | (.023) |
| Well-being | -.156*** | -.153*** | -.128*** |
| | (.030) | (.030) | (.031) |
| Observations | 4,749 | 4,749 | 4,749 |

Notes : (1) *** : significant at the 1% level, ** : significant at the 5% level, * : significant at the 10% level. (2) Robust standard errors in parentheses. (3) Baseline specifications include controls for individual and firm characteristics : age, education, occupation, marital status, difficulties to make ends meet, period of unemployment immediately before this job, establishment size, presence of an employee representative in the establishment where the person is employed, industry and country dummies. (4) Working conditions is a summary indicator of 15 adverse working conditions. (5) Psychosocial factors include job pressure, decision latitude, skill discretion and reward.

**Table 3.2** – Instrumenting perceived job insecurity

| Dependent variable : Job insecurity | Baseline<br><br><br>(1) | Baseline<br>+Work. cond.<br><br>(2) | Baseline<br>+Work. cond.<br>+Psychosoc. fact.<br>(3) |
|---|---|---|---|
| Country-specific EPRC | | | |
| Sectoral US dismissal rate | -.087*** | -.088*** | -.096*** |
| | (.029) | (.029) | (.028) |
| Controls for individual & firm characteristics | yes | yes | yes |
| Controls for working conditions | no | yes | yes |
| Controls for psychosocial factors | no | no | yes |
| Observations | 4,749 | 4,749 | 4,749 |

Notes : (1) The results shown here are obtained when estimating equation (3.5) – jointly with equation (3.4) – by conditional maximum-likelihood. The estimates are obtained when the health outcome on the left-hand side of equation (3.4) is self-rated heath. (2) *** : significant at the 1% level, ** : significant at the 5% level, * : significant at the 10% level. (3) Standard errors in parentheses clustered at the country*industry level. (4) EPRC denotes employment protection legislation (5) Individual and firm characteristics include age, education, occupation, marital status, difficulties to make ends meet, period of unemployment immediately before this job, establishment size, presence of an employee representative in the establishment where the person is employed, industry and country dummies. (6) Working conditions is a summary indicator of 15 adverse working conditions. (7) Psychosocial factors include job pressure, decision latitude, skill discretion and reward.

**Table 3.3** – IV coefficients of job insecurity

| Health outcome | Baseline | Baseline +Working conditions | Baseline +Working conditions +Psychosocial factors |
|---|---|---|---|
| | (1) | (2) | (3) |
| Self-rated health | -.689** | -.734*** | -.740*** |
| | (.311) | (.278) | (.249) |
| Backache | .178 | .224 | .207 |
| | (.488) | (.465) | (.426) |
| Skin problems | .888*** | .899*** | .862*** |
| | (.165) | (.152) | (.179) |
| Muscular pain in upper limbs | -.201 | -.141 | -.123 |
| | (.516) | (.499) | (.453) |
| Muscular pain in lower limbs | .263 | .311 | .224 |
| | (.523) | (.505) | (.476) |
| Headaches, eyestrain | .821*** | .831*** | .794*** |
| | (.177) | (.167) | (.176) |
| Stomach ache | .614 | .627* | .580 |
| | (.394) | (.382) | (.382) |
| Cardiovascular diseases | -.667 | -.623 | -.699 |
| | (.526) | (.581) | (.465) |
| Depression, anxiety | -.409 | -.393 | -.377 |
| | (.542) | (.564) | (.548) |
| Overall fatigue | -.613** | -.589* | -.558* |
| | (.308) | (.318) | (.310) |
| Insomnia, sleep difficulties | -.071 | -.042 | -.041 |
| | (.551) | (.552) | (.498) |
| Well-being | .077 | .012 | .011 |
| | (.880) | (.885) | (.854) |
| Observations | 4,749 | 4,749 | 4,749 |

Notes : (1) The results shown here are obtained when estimating equation (3.4) – jointly with equation (3.5) – by conditional maximum-likelihood. (2) *** : significant at the 1% level, ** : significant at the 5% level, * : significant at the 10% level. (3) Standard errors in parentheses are clustered at the country*industry level. (4) Baseline specifications include controls for individual and firm characteristics : age, education, occupation, marital status, difficulties to make ends meet, period of unemployment immediately before this job, establishment size, presence of an employee representative in the establishment where the person is employed, industry and country dummies. (5) Working conditions is a summary indicator of 15 adverse working conditions. (6) Psychosocial factors include job pressure, decision latitude, skill discretion and reward.

# Appendix

## A-3.1. Tables

**Table A-3.1** – Sample restriction

| Type of sample restriction | Number of observations dropped | Remaining number of observations |
|---|---|---|
| No restriction | 0 | 43,816 |
| No information on EPL (12 countries) | 12,295 | 31,521 |
| Drop ind. working in non-business sectors[a] | 12,643 | 18,878 |
| Drop self-employed ind. | 3,534 | 15,344 |
| Drop ind. working less than 15 hours | 473 | 14,871 |
| Drop public sector, public-private sector and NGO | 2,234 | 12,637 |
| Drop fixed-term contracts and temporary contracts | 2,845 | 9,792 |
| Missing values for job insecurity | 529 | 9,263 |
| Drop women | 3,722 | 5,541 |
| Unique sample[b] | 792 | 4,749 |

Notes : [a] Agriculture, mining and fuel are excluded too because of problems of data reliability. [b] "Unique sample" denotes a sample with no missing values on any of the control variables.

**Table A-3.2** – Descriptive statistics : Individual and firm characteristics, working conditions and psychosocial factors.

| | Mean (1) | Standard deviation (2) |
|---|---|---|
| Job insecurity (standardised) | 0 | (1) |
| Age | 40.93 | (11.09) |
| *Education* | | |
| Higher education | .29 | (.45) |
| Secondary education | .66 | (.47) |
| Below secondary | .05 | (.22) |
| *Occupation* | | |
| Managers and professionals | .17 | (.38) |
| Technicians and supervisors | .14 | (.35) |
| White collars | .18 | (.38) |
| Blue collars | .51 | (.50) |
| *Marital status* | | |
| Lives with a spouse or partner | .71 | (.45) |
| Difficulties to make ends meet | .35 | (.48) |
| *Establishment size* | | |
| Less than 10 employees | .28 | (.45) |
| Betweeen 10 and 49 employees | .33 | (.47) |
| Between 50 and 99 employees | .12 | (.32) |
| Between 100 and 499 employees | .17 | (.38) |
| More than 500 employees | .09 | (.29) |
| Period of unemployment immediatly before this job | .07 | (.26) |
| Presence of an employee representative | .48 | (.50) |
| Bad working condition index (0 to 10) | 3.24 | (2.90) |
| Job pressure index (0 to 10) | 4.45 | (2.42) |
| Decision latitude index (0 to 10) | 6.48 | (3.94) |
| Reward index (0 to 10) | 5.05 | (2.32) |
| Skill discretion | .71 | (.45) |
| Observations | 4,749 | 4,749 |

Notes : (1) Standard deviations in parentheses.

**Table A-3.3** – Descriptive statistics : Health variables.

| | Mean (1) | Standard deviation (2) |
|---|---|---|
| Good self-rated health | .78 | (.41) |
| Backache | .47 | (.50) |
| Skin problems | .08 | (.27) |
| Muscular pain in upper limbs | .43 | (.50) |
| Muscular pain in lower limbs | .30 | (.46) |
| Headache, eyestrain | .34 | (.47) |
| Stomach ache | .12 | (.32) |
| Cardiovascular diseases | .05 | (.21) |
| Depression, anxiety | .08 | (.27) |
| Overall fatigue | .34 | (.47) |
| Insomnia, sleep difficulties | .18 | (.39) |
| Well-being | .93 | (.25) |
| Observations | 4,749 | 4,749 |

Notes : (1) Standard deviations in parentheses. (2) All variables are binary so that the mean can be interpreted as the average frequency in the sample.

**Table A-3.4** – Descriptive statistics : Countries and industries.

| Country | Frequency(%) | Industry | Frequency(%) |
|---|---|---|---|
| Austria | 3.35 | Food and beverages | 3.92 |
| Belgium | 13.86 | Textiles, wearing app. and leather | 1.45 |
| Czech Republic | 2.80 | Wood and wood products | 1.20 |
| Denmark | 4.63 | Paper, printing and publishing | 2.13 |
| Estonia | 2.48 | Chemicals and chemical products | 2.27 |
| Finland | 3.81 | Rubber and plastics | 1.47 |
| France | 10.80 | Non-metallic mineral products | 1.24 |
| Germany | 10.44 | Basic metals and fabricated metal | 5.26 |
| Greece | 2.46 | Machinery | 3.20 |
| Hungary | 3.87 | Electrical and optical equipment | 2.55 |
| Ireland | 2.17 | Transport equipment | 3.26 |
| Italy | 4.25 | Manufacturing, recycling | 2.82 |
| Netherlands | 3.05 | Electricity, gas and water supply | 2.23 |
| Norway | 3.94 | Construction | 15.46 |
| Poland | 3.58 | Motor trade and repair | 5.41 |
| Portugal | 3.50 | Wholesale trade | 5.05 |
| Slovak Republic | 2.88 | Retail trade | 10.17 |
| Slovenia | 3.92 | Hotels and restaurants | 4.61 |
| Spain | 3.33 | Transport and storage | 8.82 |
| Sweden | 3.24 | Post and telecommunications | 1.79 |
| Turkey | 3.26 | Financial intermediation | 4.38 |
| United Kingdom | 4.36 | Real estate activities | 1.03 |
| | | Renting and business activities | 10.25 |
| Observations | 4,749 | | 4,749 |

**Table A-3.5** – Employment Protection Legislation Index (EPRC) in Europe (2008) and industry-level US dismissal rates (mean value for 2000-2006).

| Country | EPRC index | Industry | US dismissal rate |
|---|---|---|---|
| Austria | 2.62 | Food and beverages | 2.83 |
| Belgium | 2.42 | Textiles, wearing app. and leather | 6.06 |
| Czech Republic | 2.79 | Wood and wood products | 5.16 |
| Denmark | 2.06 | Paper, printing and publishing | 3.61 |
| Estonia | 2.69 | Chemicals and chemical products | 3.22 |
| Finland | 2.23 | Rubber and plastics | 3.28 |
| France | 2.37 | Non-metallic mineral products | 3.47 |
| Germany | 3.21 | Basic metals and fabricated metal | 4.08 |
| Greece | 2.59 | Machinery | 4.76 |
| Hungary | 2.19 | Electrical and optical equipment | 5.93 |
| Ireland | 1.82 | Transport equipment | 3.08 |
| Italy | 2.66 | Manufacturing, recycling | 4.58 |
| Netherlands | 2.80 | Electricity, gas and water supply | 1.78 |
| Norway | 2.43 | Construction | 5.09 |
| Poland | 2.51 | Motor trade and repair | 2.67 |
| Portugal | 3.52 | Wholesale trade | 3.80 |
| Slovak Republic | 2.86 | Retail trade | 2.98 |
| Slovenia | 3.07 | Hotels and restaurants | 2.99 |
| Spain | 2.65 | Transport and storage | 3.35 |
| Sweden | 3.11 | Post and telecommunications | 4.16 |
| Turkey | 2.51 | Financial intermediation | 2.56 |
| United Kingdom | 1.62 | Real estate activities | 2.06 |
| | | Renting and business activities | 4.19 |

**Table A-3.6** – Probit model : Self-rated health and job insecurity – Coefficients on control variables.

| | Dependent variable : Dichotomised Self-Rated Health | |
|---|---|---|
| | Coeff (1) | S.e (2) |
| Job insecurity | -.077*** | (.023) |
| Age | -.027*** | (.002) |
| *Education (Ref : Below secondary)* | | |
| Higher education | .443*** | (.122) |
| Secondary education | .509*** | (.109) |
| *Occupation (Ref : Blue collars)* | | |
| Managers and professionals | .074 | (.084) |
| Technicians and supervisors | -.013 | (.083) |
| White collars | .045 | (.075) |
| *Marital status (Ref : Does not live with a spouse nor a partner)* | | |
| Lives with a spouse or partner | -.036 | (.052) |
| Difficulties to make ends meet | -.289*** | (.052) |
| Period of unemployment immediately before this job | .167* | (.090) |
| *Establishment size (Ref : Less than 10 employees* | | |
| Betweeen 10 and 49 employees | -.078 | (.059) |
| Between 50 and 99 employees | -.050 | (.081) |
| Between 100 and 499 employees | .031 | (.077) |
| More than 500 employees | -.167* | (.095) |
| Presence of an employee representative | -.021 | (.053) |
| Bad working condition index | -.067*** | (.009) |
| *Job pressure (Ref : High job pressure)* | | |
| Low job pressure | .217*** | (.059) |
| Moderate job pressure | .051 | (.056) |
| *Decision latitude (Ref : Low decision latitude)* | | |
| High decision latitude | .156*** | (.056) |
| Moderate decision latitude | -.013 | (.064) |
| *Reward (Ref : Low reward)* | | |
| High reward | .473*** | (.070) |
| Moderate reward | .293*** | (.051) |
| Skill discretion | .002 | (.053) |
| Controls for country dummies | yes | yes |
| Controls for industry dummies | yes | yes |
| Pseudo R-squared | 0.16 | |
| Observations | 4,749 | |

Notes : (1) *** : significant at the 1% level, ** : significant at the 5% level, * : significant at the 10% level. (2) Robust standard errors in parentheses.

**Table A-3.7** – Instrumenting perceived job insecurity – Coefficients on control variables

| | Dependent variable : Job insecurity | |
|---|---|---|
| | Coeff | S.e |
| | (1) | (2) |
| Sectoral US dismissal rate*country-specific EPRC | -.096*** | (.028) |
| Age | -.001 | (.001) |
| *Education (Ref : Below secondary)* | | |
| Higher education | .078 | (.080) |
| Secondary education | .027 | (.075) |
| *Occupation (Ref : Blue collars)* | | |
| Managers and professionals | .080* | (.048) |
| Technicians and supervisors | -.005 | (.049) |
| White collars | .047 | (.050) |
| *Marital status (Ref : Does not live with a spouse nor a partner)* | | |
| Lives with a spouse or partner | -.091*** | (.029) |
| Difficulties to make ends meet | .235*** | (.031) |
| Period of unemployment immediately before this job | .071 | (.058) |
| *Establishment size (Ref : Less than 10 employees* | | |
| Betweeen 10 and 49 employees | -.052 | (.040) |
| Between 50 and 99 employees | -.085 | (.052) |
| Between 100 and 499 employees | -.024 | (.050) |
| More than 500 employees | -.124** | (.063) |
| Presence of an employee representative | .016 | (.031) |
| Bad working condition index | .007 | (.007) |
| *Job pressure (Ref : High job pressure)* | | |
| Low job pressure | -.197*** | (.034) |
| Moderate job pressure | -.153*** | (.034) |
| *Decision latitude (Ref : Low decision latitude)* | | |
| High decision latitude | -.134*** | (.036) |
| Moderate decision latitude | -.086** | (.044) |
| *Reward (Ref : Low reward)* | | |
| High reward | -.337*** | (.039) |
| Moderate reward | -.157*** | (.034) |
| Skill discretion | .049 | (.034) |
| Controls for country dummies | yes | yes |
| Controls for industry dummies | yes | yes |
| Observations | 4,749 | |

Notes : (1) The results shown here are obtained when estimating equation (3.5) – jointly with equation (3.4) – by conditional maximum-likelihood. The estimates are obtained when the health outcome on the left-hand side of equation (3.4) is self-rated heath. (2) *** : significant at the 1% level, ** : significant at the 5% level, * : significant at the 10% level. (3) Standard errors in parentheses clustered at the country*industry level.

# General Conclusion

This thesis establishes several results on career shocks and their impact on health. We present the main results obtained, before turning to their limitations. We conclude by suggesting several implications in terms of public policy.

## Main results

**Does leaving school in a bad economy deteriorate health?** We show that leaving school in a bad economy is an early socio-economic risk with long-lasting consequences on health – in particular among low-educated women. Our results show that women who faced higher unemployment rates at labour-market entry during the 1970s crisis in the UK were in worse health during the whole period under study (1983-2001). They had a higher probability of reporting poor health and of consulting a GP over the whole period, i.e. from 7 to 26 years after school-leaving. Additional results suggest that they were also more likely to suffer from long-standing illnesses. Our results for men are more mixed, ranging from health-damaging effects to insignificant ones, depending on the specification used. However, we never find a positive effect of leaving school in a bad economy on men's health. Overall, we provide evidence that the low-educated – at least women – are strongly affected by poor economic conditions at labour-market entry, in line with previous results by Cutler et al. (2015). Importantly, we provide evidence that our findings are not biased by endogeneous timing.

**Gaining weight through retirement?** At the other end of the age spectrum, we show that retirement acts as a final career shock for the most vulnerable individuals. Men in strenuous jobs and already at risk of obesity are at a higher risk of becoming obese following retirement (two to four years after retirement). Women's weight is not affected by retirement. We provide evidence that our results are not driven by selection into retirement. Given the increasing number of people approaching retirement age and the upward trend in obesity rates (where each cohort is heavier than the previous one), men already at risk of obesity and retiring from strenuous jobs will be likely to suffer from health disorders in the near future – especially as obesity is a major risk factor for cardiovascular diseases among men in their sixties.

**Is job insecurity harmful to health?** In-between these two critical periods, we focus on prime-age workers who hold permanent contracts. We show that even an anticipated career shock can be health-damaging. More specifically, we find that the fear of involuntary job loss triggers mild health symptoms, such as headaches or eyestrain and skin problems. Once again, our results are not driven by selection effects or other potential endogeneity biases. Although job insecurity is not a highly traumatising event such as job loss, it is much more widespread, and our results show that its impact is not trivial.

## Limitations

**Mechanisms and heterogeneity.** Each chapter has produced a causal estimate of a different career shock – or anticipated career shock – on health. A common limitation to these chapters is that the mechanisms by which career shocks translate into worse health are not clear, at least from an empirical point of view. This is due both to data limitations and methodological issues. In the first chapter, the mechanisms by which poor economic conditions at labour-market entry cause worse health in the long-term are not fully understood. Our results are consistent with the "initial shock" hypothesis, according to which high labour-market uncertainty at school-leaving leads to greater stress and triggers addictive behaviours and mental disorders in the short-run. In this scenario, health falls immediately after school-leaving, and this fall is not compensated over the lifecourse. But, our empirical findings are also consistent with the "cumulative effect" hypothesis. In this scenario, poor economic conditions at school-leaving lead to lower life-time earnings and job quality. The negative health impact of lower wages and lesser job quality cumulates over the lifecourse and generates health disparities in the long-run. Overall, our results do not permit to determine which hypothesis – the initial effect or the cumulative one – is most likely to prevail in the data. Data over the whole lifecourse – before and after school-leaving – would help, of course; however, this kind of data is rare. In any case, modelling the link between work and health from the time individuals leave school is not an easy task. This is mainly due to endogeneity issues – reverse causality between health and work, typically – that plague the analysis. Overall, the mechanisms by which poor economic conditions at school-leaving have a lasting and negative impact on health are not understood. Answering this question, however, would be crucial to develop efficient mitigation strategies.

Our second chapter makes an attempt to explore the mechanisms by which retirement causes obesity. It investigates the respective role of food consumption and physical activity. The results, however, are somewhat disappointing, mainly because of data limitations. Only very precise measures of food intake or physical activity would have been useful to our purpose. Such data are usually not available in surveys, and SHARE is no exception. A way to investigate the potential mechanisms at play in the retirement-obesity connection is to split the sample according to some relevant characteristics. Typically, we split our sample by occupational strenuousness before retirement and find that our results are driven by men who retired from strenuous jobs. This indicates that job-related physical activity before retirement is an important channel through which retirement affects health. However, splitting our sample by potentially endogeneous characteristics – e.g. being in a strenuous job – is problematic. Men may indeed select into strenuous jobs according to some specific characteristics. To the extent that this selection is dynamic, our results will be biased.

The last chapter hypothesises that the mechanisms by which job insecurity leads to worse health include stress as well as the increase in precautionary savings. Our data do not enable us to test for this, empirically : the EWCS survey contains few reliable measures of stress and no information whatsoever on health consumption. We argue that whatever the mechanism through which perceived job insecurity affects health, this effect is likely to be stronger for workers with low employability, that is, with a low probability of finding a new job if losing the current one. To investigate further this matter, one would like to split the sample according to individuals' employability. Unfortunately, finding an exogeneous measure of employability – i.e. not related to worker's characteristics – is not an easy task. In particular, the information available in the EWCS database does not allow us to tackle this issue properly. Overall, the heterogeneous impact of job insecurity on health according to employability cannot be dealt with properly, mainly because of data limitations and methodological issues. This is a limitation to our study to the extent that the policy implications would not be the same if (i) individuals were negatively affected by job insecurity, regardless of their employability or (ii) if only workers with low employability were negatively affected by job insecurity. Only in the second scenario would policies aiming at improving the employability of individuals (e.g. through active labour-market policies) smooth out the effects of job insecurity.

## Policy relevance

My thesis has several implications in terms of public policy.

Generally speaking, it shows that at least some part of the relationship between careers and health reflects causation from careers to health. To provide such causal evidence is important from a policy point of view, so that policies directed at securing careers or at increasing the quality of jobs can be empirically grounded. In particular, our results suggest that policy makers should pay attention to critical periods – such as the first entry on the labour-market or retirement.

Our first chapter suggests that policies that target youth unemployment might have particularly large payoffs over the long term in reducing health disparities, especially among the low-educated. Whether job training and other programs really improve the labour-market success of young low-educated individuals is debated in the literature (Card et al., 2011). Cutler et al. (2015) suggest that (i) the evaluation of such programmes should include health and health behaviours as outcomes (ii) and that non-labour market programs could help disadvantaged youth in bad economic times by, for instance, improving mental health and preventing the development of poor health habits. Of course, the extent to which our results can be generalized to the current context – and in particular to young people who entered the labour market during the Great Recession – is a relevant issue. Our results hold for individuals who left school at compulsory age in the 1970s. By then, 50% of pupils left school at compulsory age, while less than 20% do so nowadays. Moreover, there is some evidence that the 1973 oil crisis and the current Great recession did not have the same effects on unemployment rates, wages and working conditions in the UK Gregg and Wadsworth (2011). To the extent that we focus on the long-term health impact of leaving school in a bad economy, however, we have no choice but exploiting an historical natural experiment – even if the external validity of the results is at stake.

The second chapter suggests that men already at risk of obesity and retiring from strenuous jobs will be likely to suffer from health disorders in the near future – as obesity has a particularly strong impact on morbidity and disability among adults aged 50 and older (Peytremann-Bridevaux and Santos-Eggimann, 2008; Jenkins, 2004; Andreyeva et al., 2007) and is a major

risk factor for cardiovascular diseases among men in their sixties. From an inequality perspective, this heterogeneous impact of retirement may exacerbate post-retirement weight and health disparities, as retirement seems to affect the most vulnerable individuals – men in strenuous jobs and at risk of obesity. Public health policies specifically targeted at this population should be considered in order to guarantee healthy ageing and healthy life years following retirement. From a more general perspective, recent evidence suggests that while disability prevalence has declined in recent years among the "oldest old", it is increasing among the "young old" (Freedman et al., 2013; Martin et al., 2010). One recent trend that could affect both morbidity and mortality among future cohorts is the increasing rate of obesity (Hudson, 2005). This upward trend in obesity rates combined with an increasing number of men approaching retirement age could jointly explain this phenomenon.
Our last chapter considers individuals who hold a permanent contract and investigate whether the fear of involuntary job loss is harmful to health. We show that job insecurity triggers mild health symptoms – such as skin problems, headaches or eyestrain. This is quite worrying, as the perception of job insecurity has steadily increased in most OECD countries since the 1990s. We argue that the impact of job insecurity is likely to be stronger for workers with low employability, i.e with a low probability of finding a new job if losing the current one. Although we do not provide any empirical evidence of this, this suggests that the impact of job insecurity is likely to be particularly strong in a country with a dual labour market like France[36], where job loss – especially at advanced ages – is a dangerous event, which entails a high risk of downward mobility (see on this topic the essay by Eric Maurin *La Peur du Déclassement* (Maurin, 2009)).

Finally, it is particularly interesting to interpret our results in a context where populations are rapidly ageing. Cohorts who graduated in the 1970s underwent several recessions and had more insecure careers (high unemployment rates, more frequent temporary employment etc.) than previous ones. They are now ageing and close to retirement. If the cumulative effect of insecure careers across the lifecourse is particularly harmful to health, it will be problematic for two reasons : (i) post-retirement health will be poor, especially if retirement acts as a final shock on most vulnerable individuals – e.g. men in strenuous jobs and at risk of obesity (ii) measures aiming at increasing the economic activity of theses cohorts will not necessarily be effective, especially if ill health is a motive of labour market withdrawal at old age.

[36]See Le Barbanchon and Malherbet (2013) on this topic. The emergence of dual employment protection can be broadly defined as the coexistence of both long-term contracts, which benefit from stringent protection, and short-term contracts with little or no protection.

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**Titre : Trajectoires professionnelles et santé en Europe**

**Résumé :** Cette thèse se propose d'analyser les effets des ruptures dans les trajectoires professionnelles sur l'état de santé des individus en Europe. Nous considérons ici deux ruptures : l'une en début de carrière – l'entrée sur le marché du travail dans une économie dégradée – et l'autre en fin de carrière – le passage à la retraite. Entre ces deux périodes critiques, nous portons un intérêt spécifique à l'impact sur la santé d'une rupture cette fois anticipée : la peur de perdre son emploi. Nos analyses empiriques combinent des données d'enquêtes Européennes et Britanniques. Afin de pallier les problèmes d'endogénéité propres à toute analyse empirique du lien entre santé et trajectoire professionnelle, nous exerçons des chocs exogènes sur la carrière des individus. Nous utilisons ainsi une expérience naturelle (la crise pétrolière de 1973) et les caractéristiques institutionnelles telles qu'elles sont définies dans la législation de chaque pays Européen (âges légaux de passage à la retraite, degrés de protection de l'emploi, règles de scolarité obligatoire). Les résultats soulignent l'effet néfaste des ruptures au cours de la vie professionnelle sur la santé des individus, à la fois à court et à long terme.

**Mots clés :** Santé, Retraite, Insécurité de l'emploi, Chocs Macro-économiques, Obésité.

**Title : Essays on careers and health in Europe**

**Abstract :** The main objective of this thesis is to analyse the health consequences of career shocks in Europe. It considers two *actual* career shocks over the lifecourse : leaving full-time education in a bad economy, and, at the other end of the age spectrum, retiring. In-between these two critical periods, it investigates how an *anticipated* career shock – i.e. anticipated job loss – damages health. Empirical analyses are conducted using large European and British surveys. We use institutional features and natural experiments to find neat instruments for causal identification : the existence of compulsory schooling laws, the cross-country variation in employment protection legislations, the cross-country variation in retirement systems and the 1973 oil crisis. The results emphasise the causal and health-damaging impact of career shocks, both in the short and in the long-term.

**Keywords:** Health, Retirement, Job insecurity, Macro-economic shocks, Obesity.